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ORIGINAL ARTICLES.

I.—Eminent Living Geologists: Prof. Albert Jean Gaudry, Memb. Inst. France, For. Memb. R.S., For. Memb. Geol. Soc.

(WITH A PORTRAIT, PLATE II.)

THE recent retirement of Professor Albert Jean Gaudry, who for fifty years has been associated with the Jardin des Plantes, gives us an opportunity of presenting our readers with his portrait.

Born in 1827, he started for Cyprus and Greece at the age of 25, on what was destined to prove an epoch-making visit, for while there he obtained satisfactory information about the rich deposit of vertebrata at Pikermi, which he afterwards visited. The results of his labours are embodied in the classic "Animaux fossiles et Géologie de l'Attique" (1862–67), and a new world of vertebrate life was fully opened to zoologists, of which the first glimpse had been obtained by Wagner. But perhaps the best known of Gaudry's writings is his "Enchaînements du monde animal dans les temps géologiques" (1878), reprinted in 1895, a work which has had great influence on the younger school of thought (see Geol. Mag., 1878, pp. 221–227, and 1884, pp. 32–40).

Appointed Assistant to Professor Alcide d'Orbigny (the first to hold the Chair of Palæontology at the Jardin des Plantes) in 1853, while still in Cyprus, he succeeded Edouard Lartet in that Chair in 1872. He was Vice-Secretary of the Geological Society of France in 1852, Secretary in 1854, and President in 1863, 1878, and 1887; while in 1900 he presided over the Meetings of the Eighth International Congress of Geology, held in Paris.

Gaudry's first published works dealt with Starfishes (1852) and the Origin of Flint (1852), but soon afterwards he confined himself to Vertebrate Palæozoology, with a result that is known to even the most superficial student of the science throughout the world.

Professor Gaudry has observed that, among students of Nature of all ages, two methods of work may be noticed: the *synthetical* and the *analytical*. It is not difficult to see that he himself is more in sympathy with the former, and on the occasion of his Jubilee, recently celebrated in Paris (Geol. Mag., May, 1902, p. 240), the

bent of his mind, as reflected in his writings, was dwelt upon by MM. Perrier, Boule, and Liard, the speakers on that occasion.[1]

Applause is too often readily bestowed upon speculative work, which deserves no higher praise than that of being happily conceived; at the time no one enquires whether there is any solid basis for the speculation, and the writer is often thoroughly satisfied with his temporary success. It is different with Gaudry. The real and lasting merit of his work is due to the fact that his generalizations are based upon painstaking and accurate analytical research; analysis and synthesis are happily blended, and his generalizations are the outcome of careful analytical investigation. This is not only true of his descriptive publications ("Animaux fossiles et Géologie de l'Attique"; "Animaux fossiles du Mont Léberon"), but also of those which are professedly of a more philosophical nature, and among the latter his "Enchaînements" certainly takes the first place. In this work the generalizations are not merely based on previously known data, but upon numberless original observations made known for the first time in that volume.

It should also be borne in mind that from his earliest writings Professor Gaudry, like his illustrious contemporary, Professor Rütimeyer, of Basel University, Switzerland, based his teaching on the theory of evolution, a fact which made his work, as Boule reminds us, an act of independence and courage which few men of science on the Continent were prepared to follow in those early days of Darwin's writings.

Professor Gaudry's separate contributions to palæozoology in various scientific journals numbered 85 up to 1883, but many others have appeared during the past 20 years. Among those of later years we may mention two of special importance, namely, his memoir on *Actinodon Frossardi*, Gaudry, discovered by M. Bayle in the Lower Permian, Autun, Saone et Loire, France (Nouvelle Archives du Museum, ser. II, vol. x, 1887, pp. 9–19, pl. i); and his account of the discovery and reconstruction of the entire skeleton of *Elephas meridionalis*, F. Nesti, in the Volume Commémoratif Centenaire de la Fondation du Musée d'Histoire Naturelle, Paris (4to, 1893, pp. 327–348, with plate).

Professor Gaudry was elected a Foreign Correspondent of the Geological Society of London in 1868, and a Foreign Member in 1874; and at the annual general meeting on February 15th, 1884, the President, Mr. J. W. Hulke, F.R.S., in presenting him with the Wollaston Gold Medal, the highest honour the Society is able to confer, addressed him as follows:—"Professor Gaudry, the Council of the Geological Society has awarded you the Wollaston Medal in recognition of the value of your palæontological researches and the important scientific generalizations you have deduced from long and laborious observations. The numerous papers on topographical geology and on palæontology you have contributed during the past 30 years, your important 'Recherches Scientifiques en Orient entreprises par les ordres du

[1] "Jubilé de M. Albert Gaudry" (Paris, par le Comité, 1902).

Gouvernement pendant les années 1853–1854,' your 'Animaux fossiles et Géologie de l'Attique,' and, lastly, your work 'Les Enchaînements du monde animal dans les temps géologiques,' have made your name so familiar, wherever our branch of natural science is cultivated, that in receiving you we feel we are not receiving a stranger, but a scientific brother, and one who, by his labours and singleness of aim, has achieved a position as a palæontologist such as few can hope to attain. Personally it affords me great and sincere pleasure that it has fallen to my lot to hand you this Medal, which, by the consent of all, has never been more worthily bestowed." Later on the Royal Society recognized Professor Gaudry's merits by electing him one of its Foreign Members in 1895.

But the crowning honour of Professor Gaudry's career was reserved for the 9th March last, when his pupils, friends, and admirers assembled in the Palæontological Gallery, one of the most beautiful buildings of the Museum of Natural History in the Jardin des Plantes, to commemorate the fiftieth anniversary of his connection with the Museum by presenting to him a gold medal, designed by M. F. Vernon, bearing his portrait and name on the obverse, and on the reverse a classical female figure (to represent our science), supporting a geological pickaxe, beside which rest the skull and lower jaw of *Dinotherium*, and at her feet numerous fossil bones. In the background is a view, in low relief, of the ravine and hills at Pikermi, Greece, where Gaudry carried out his great work nearly fifty years ago. This side bears the inscription above—

"A . Albert . Gaudry
Paleontologiste
ses . Eleves . ses . Amis
ses . Admirateurs";

and beneath the figure the words

"Pikermi . 1855–1860."

Over forty French and foreign Academies and scientific bodies either sent delegates or forwarded written addresses, and nearly 350 names of distinguished men of science of all nationalities have subscribed to the list.

Apart from his eminent scientific attainments which have rendered his name so well known and honoured as one of the leading biologists of the day, Professor Gaudry has endeared himself, not only to his pupils and colleagues, but to a very wide circle of friends and admirers, by the amiability of his disposition and the kindness of heart which he has always displayed both to friends and strangers, at all times, whenever they have approached him and sought his personal or scientific aid. Long may he live and continue to enjoy that warm regard which all feel for so amiable and kindly a man.

His former pupil and valued assistant of many years, Professor Marcellin Boule, has, we learn, been appointed as successor to Professor Gaudry, and will carry onward with him in his career the same good wishes which follow his master into his retirement.

II.—Additional Note on the Correlation of the Rocks associated with the Devonian Limestones of the Hindu Khoosh.

By Lieut.-General C. A. McMahon, F.R.S., F.G.S.

SINCE the publication of the joint paper by Mr. W. H. Hudleston and myself on "Fossils from the Hindu Khoosh,"[1] in which the Devonian age of well-preserved fossils found in the limestone member of a series in Chitral was demonstrated, my attention has been called by Mr. T. H. Holland to the fact that the series in Chitral agrees very closely with the rocks known to the Geological Survey of India as the infra-Trias, and described, for the last time, in Hazara[2] by Mr. C. S. Middlemiss.

The Chitral series consists of three principal formations:—(1) A lower bed of conglomerate with rounded to subangular pebbles, varying greatly in size up to $3\frac{1}{4}$ inches in diameter, lying in an indurated, fine-grained matrix of slaty grit or arenaceous mudstone. The pebbles consist of limestone, slates, sandstones, and quartzites, with rounded, white quartz-pebbles, which recall the 'eggs' of the Blaini conglomerate of the Simla area. (2) A middle band described by my son, who made a hurried visit to the place, as red sandstone; and (3) an upper bed of grey, dark-blue, and cream-coloured fossiliferous limestone.

In Hazara the system of rocks known to the Geological Survey as the infra-Trias, on account of its position unconformably below the Trias, consists in the same way of a lower conglomerate, a middle sandstone series, and an upper limestone formation. These rocks, originally referred to by Wynne and Waagen, were described in fuller detail by Middlemiss in 1896.[3]

According to Middlemiss, the conglomerate is composed of subangular pebbles of slates and quartzites, usually of about the size of a cricket ball, but varying from mere pebbles to larger lumps, and set in a fine purple sandy clay or shale. Like the Chitral conglomerate the bed—boulders and matrix—in Hazara has undergone a certain amount of metamorphism. The middle member of the series in Hazara is composed of purple shale and sandstone, the latter predominating. The limestone in Hazara attains a thickness of 2,000 feet, which, in the lowest layers, is sandy and of a deep purple colour, passing up into more purely calcareous beds of a lighter colour and even pink, cream-colour, or white. But the limestones are generally purplish or pink in colour, thus differing from the fossiliferous limestones of Chitral and resembling that of the Blaini area, which also is generally pink, though occasionally grey or blue. No fossils have been found in the Hazara limestone, but Middlemiss suggests that they may have been destroyed by metamorphism.

[1] Geol. Mag., Dec. IV, Vol. IX, pp. 3–8, 49–58, January and February, 1902.

[2] Hazara is about 130 miles S.S.E. of Chitral. The rocks in both areas have a general foliation-strike of about N.E.–S.W.; so there is no means of correlating them, except by general lithological correspondence, until the intermediate ground is surveyed in detail.

[3] C. S. Middlemiss, "The Geology of Hazara and the Black Mountain": Mem. Geol. Surv. Ind., vol. xxvi, p. 17 ff. (1896).

Thus, whilst the order of succession is the same in both areas, namely, conglomerate, sandstone series, and limestones, there are small differences, due possibly to a less thorough examination of Chitral, where the rocks were merely seen on a flying visit to a country very difficult of access. These differences consist mainly of differences of colour, which in Hazara is a prevalent purple through all three formations, whilst in Chitral only the sandstone is referred to as red. As such differences of colour are more probably due to secondary than primary causes, I think that their importance is not great when compared with the general similarities of the two series.

In referring to this case of Hazara I wish to point out that Mr. Middlemiss, after describing the infra-Trias in that area, and after having seen the well-known Blaini beds near Simla, had no hesitation in correlating the conglomerate of Hazara with the Blaini boulder bed. As it seems probable that the infra-Trias conglomerate of Hazara is the equivalent of the conglomerate below the Devonian limestone of Chitral, we have a further support for the correlation of the Chitral conglomerate with that of the Blaini beds, and therefore a further reason for regarding the Blaini conglomerate as at least Devonian in age.

There is, however, one element of doubt about the correlation of conglomerates in isolated areas, which must necessarily show certain points of correspondence, and that is illustrated by the fact that Mr. Middlemiss has also correlated the Hazara conglomerate with the boulder bed of the Salt Range, whose age, as shown by the associated fossils, is not greater than Upper Carboniferous. But at the same time he recognised certain differences, the chief being the absence of striated pebbles in Hazara, and it is in this important respect that the conglomerates of Hazara, Chitral, and Blaini all agree to differ from the boulder beds of the Salt Range and the Talchirs, whose age has been determined by fossil evidence.

In default, therefore, of evidence to the contrary we are justified in regarding the Chitral conglomerate as the horizon of reference for these similar conglomerates in the old unfossiliferous systems of the North-West Himalayas, whilst the boulder beds of the Salt Range, the Talchirs, and their foreign equivalents, which so often show direct evidence of ice-action, are younger in age.

III.—Neolithic Flint Implements from the Northern Desert of the Fayûm, Egypt.

By H. J. L. Beadnell, F.G.S., F.R.G.S., of the Geological Survey of Egypt.

(PLATES III AND IV.)

IN the course of my work in the deserts of the Fayûm during the last two years I found that over a certain area to the north of the eastern end of the Birket el Qurun lake flint implements, many of very fine workmanship, were of frequent occurrence. The accompanying Plates illustrate the common and most perfect types (reduced to about one-third).

For some years the existence of flint implements in the Fayûm has been known, as specimens of the more common forms had been collected by Beduin and sold to antiquity dealers in Cairo, in whose shops they were exposed for sale. Nothing, however, appears to have been known as to their exact locality and mode of occurrence, and, with the exception of one or two specimens figured by M. de Morgan, the types of this group have never been illustrated or described. The accompanying Sketch-map shows the particular part of the Fayûm desert whence these implements were obtained.

It is not intended to describe in detail the different specimens figured, or to attempt to divide them too closely into different classes; the chief varieties may be briefly referred to as follows:—

A. *Arrow and Spear Heads.* (Plate III, Figs. 1–9.)—These occur in a great variety of forms, the untanged sort being especially noticeable; the outer edge is frequently serrated. The finished specimens are usually of very fine workmanship. Probably they were largely used for spearing fish, as I shall show that these flint-using people lived on the borders of a great lake teeming with fish.

B. *Saw-edged Implements.*—The common varieties are seen in Plate III, Figs. 10–14.

C. *Cutting-edged Implements.*—In Plate III, Figs. 15 and 16, and Fig. 7 in Plate IV are spear-shaped forms, while Figs. 17–21 in Plate III and Figs. 1, 3–6 in Plate IV may be called knives, and were probably used for a variety of purposes. Fig. 2 is a curiously-shaped weapon.[1]

D. *Axe-like Tools.* (Plate IV, Figs. 8–11.)—The specimen shown in Fig. 8 is typically 'neolithic' in type, being beautifully ground and having a very true, sharp, cutting edge. The other three are also well-made tools, and show a combination of flaking and grinding. In addition to the above more or less definitely marked types, are numerous more rudely-shaped flints, many probably unfinished discarded failures, which do not lend themselves to classification. In one or two places the number of flakes lying together suggested that the work of fabrication had been done on the spot, but no extensive workings have as yet been noticed. Several varieties of flint and chert are noticeable, and although some of the material may have been supplied by the neighbourhood, it was probably mostly derived from the Lower Eocene limestones to the south or from the Cretaceous limestones of Abu Roash to the north.

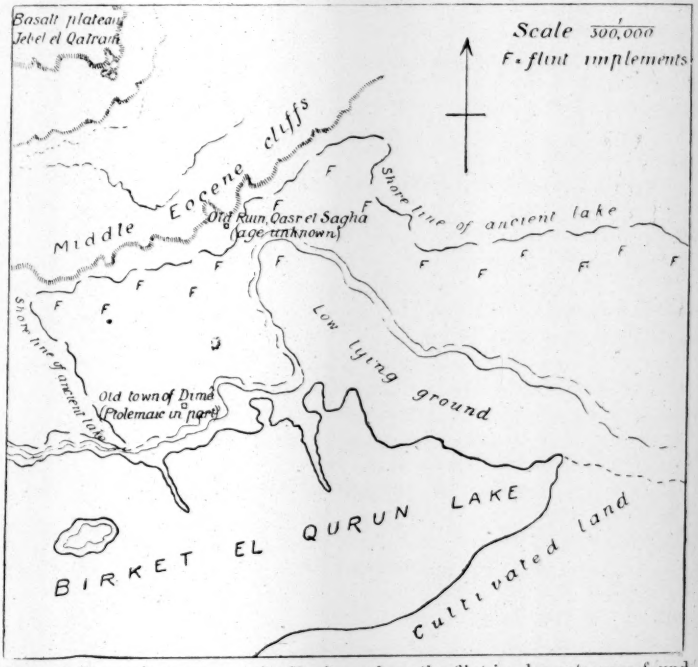
With regard to the age of these flints, Mr. Quibell informs me that it is impossible to date them by comparison with known flints from tombs, etc. There may be certain resemblances between some of them and others of known age, but the group as a whole seems quite distinct. A most interesting and important fact, however, is their connection with the ancient topography of the Fayûm. Although I have traversed the Fayûm depression in almost every direction, I have so far failed to find any of these implements except

[1 Perhaps a spearhead?—Edit.]

Flint Implements from the Fayûm, Egypt.

About one-third nat. size.

in the comparatively restricted area in the north part. This part of the desert is covered with deposits indubitably laid down on the floor of a great lake—lacustrine sands, clays and marls full of the remains of fresh-water shells, fish, hippopotami, sheep, etc. The limits of the former lake are clearly defined, and the flint implements are almost wholly restricted to this area. They are very rarely found in the surrounding desert, and are most common along the outer fringe of the ancient lake. Nearly all the figured specimens were picked up on the surface of these lacustrine deposits, which are very soft, and are being rapidly cut up by wind-borne sand. The implements in nearly all cases show no signs of staining or patination, the upper and under sides being almost indistinguishable; they are quite fresh and unworn, and little polished by sand, exhibiting in fact no evidence of having been long exposed. I think there is no doubt that they have weathered out of the deposits themselves, although they have never as yet been actually discovered *in situ*. The people who used these flints certainly lived on the borders of this lake, which teemed with fish, hippo, and crocodiles.



Map of the northern part of the Fayûm, where the flint implements were found.

The presence of these deposits is alone sufficient to show the former existence of a great lake covering the floor of the Fayûm depression. It is quite certain also that the water of the lake was derived from the high floods of the Nile in the adjoining valley,

and that the water obtained access to the depression through the low gap in the divide at Lahun. At what date the Nile water first entered the Fayûm is the crucial question, as the area must have been quite uninhabitable anterior to that event. It is, however, well known that prior to the Middle Empire a great lake existed, and was then brought under artificial control by Amenemhat I and his successors in the prosperous period of the Twelfth Dynasty (2200 B.C.). This Lake Moeris was used as a regulator of high and low Nile floods, and a certain amount of land was reclaimed from the central and highest portion of its bed; its level was between 23 and 24 metres above present sea-level, a height that agrees closely with that of the outer limit of the lacustrine deposits in the flint implement area.

The level of the lake was apparently kept at about the same level until after the time of Herodotus, about 1800 years later. If we could show that the old natural lake never reached the level of Lake Moeris in the Twelfth Dynasty, we should prove the flints to be of the same or later age. But it is probable that the old lake reached the same level soon after the first entrance of the Nile waters into the depression, which may have been, for anything I know to the contrary, thousands of years before. It must also be remembered that no flints resembling the present collection appear to have been discovered in the ancient towns and tombs of the Fayûm, so that our specimens are probably of earlier date than the most ancient monuments in the district.

The chief points on which evidence is wanted for dating these flints is as to when the Nile first gained access to the Fayûm depression, and as to the subsequent levels of the lake between that date and the Twelfth Dynasty. We must, however, regard them as Neolithic, both on account of their high perfection of workmanship and their connection with a lake which certainly existed far into the historic period of Egypt.

The evidence for Palæolithic man in Egypt appears to rest on very indefinite evidence. Throughout the country rudely worked flints may be picked up on the surface of the desert fringes, and those from the high plateaux have usually been designated 'palæoliths.' Whether any of these are in reality comparable with the 'palæoliths' of Europe is, I think, at present, an unanswerable question, but certainly in some cases their shape, workmanship, discolouration, and amount of weathering do not prove the contrary. Forbes,[1] who has recently described a large number of flints collected by Seton Karr in the Wadi el Sheikh of the Eastern desert, appears to be very much opposed to the Palæolithic view. He dates the Wadi el Sheikh implements as probably Twelfth Dynasty and later, and possibly going back to the Fourth; the evidence on which he relies appears to be the resemblance of some forms to flints discovered by Petrie in the old town of Kahun in the Fayûm. In the Wadi el Sheikh

[1] H. O. Forbes, LL.D.: Bull. Liverpool Museums, vol. ii, Nos. 3 and 4 (January, 1900), pp. 77–115.

Flint Implements from the Fayûm, Egypt.

About one-third nat. size.

the implements are found on the spot where they were worked, close to the pits from which the material was excavated. The edges of the flakes and the outlines of the flakings are "as sharp and unworn as the day they were made," that is to say, absolutely unweathered; I quite agree that a high antiquity cannot be assigned to these. But, according to the same authority, other and associated specimens are deeply eroded and rounded at the angles and edges. Dr. Forbes appears to have no doubt as to the two kinds, the worn and unworn, being of the same age, but personally I do not feel at all convinced of this, especially as no explanation is given as to why some flints have undergone so great a change while others remain absolutely fresh and unworn. The amount of weathering should be one of the most important criteria as to the length of time that an object has been exposed on the desert surface. It may be said that as all these flints were found near together around the old pits it is probable that they are all of the same age; but, if a superior quality of flint occurs in the Wadi el Sheikh, I do not see why it should not have been discovered and worked even in Palæolithic times, or why subsequent races should not have rediscovered and worked the same beds, and their products have mixed on the surface.

The fact that many of the so-called Palæolithic flints of the Theban plateau were found in close association with nodules and the flakes struck from them is, according to Forbes, conclusive evidence that they cannot be of any great age. He says, "it is impossible to believe that these could remain (even in a single instance) undisturbed from the Palæolithic days of Europe to the present time, when the forest under which they were made and the forest soil on which they reposed have been entirely carried away." But is it certain that the high plateau was then clothed with forests? What evidence is there to show that it differed in any important respect from its present aspect? And if, as I suggest, desert conditions obtained then as now, and man merely worked his flints along the edges of the plateaux overlooking the Nile Valley, I see no reason why flint implements, dating even from Palæolithic times, should not in favourable cases be still found in the spots where they were left, surrounded by the flakes struck off in manufacture. On the flat plateaux the occasional rains which fall can effect but little transport of material, and merely lower the general level by dissolving the underlying limestone, so that the plateau surface is left with a coating of nodules and blocks of insoluble flint and chert. Flint implements might thus be expected in many localities to remain for indefinite periods, but they would certainly become more or less 'patinated,' pitted on the surface, and rounded at the angles after long exposure to heat, cold, and blown sand. Flints that retain their original clearness and sharpness of angles cannot be of high antiquity, unless they have been protected by superficial deposits.

It must, I think, be admitted that most of the conclusions formed as to the age of flint implements in Egypt, other than those in

tombs, are based on insufficient evidence, and on faulty assumptions of Egyptologists as to the geological and meteorological conditions in the Pleistocene and Recent periods. For instance, Mr. Griffith[1] writes:—"Egypt, as we know it, came into existence in the Pleistocene epoch, and then began the alluvial deposit to which the richness of the soil is due. But before the formation of the Nile Valley Palæolithic man was on the ground." Again, Petrie[2] writes:—"The valley of the Nile is cut down a depth of 1,400 feet through a limestone plateau, the edges of which are deeply channelled with drainage valleys. . . . On the top of the 1,400 feet plateau are great numbers of worked flints of Palæolithic type. . . . That the high plateau was the home of man in Palæolithic times is shown by the worked flints lying scattered around the centres where they were actually worked. The Nile, being far higher then, left no mud flats as at present for habitation, and the rainfall, as shown by the valley erosion and waterfalls, must have caused an abundant vegetation on the plateau where man would live and hunt his game."

What evidence is there that Palæolithic man was on the ground before the formation of the Nile Valley? In all probability the valley came into existence even long before the earliest times of European Palæolithic man. Again, the presence "of worked flints lying scattered around the centres where they were actually worked" does not necessarily prove that the plateau was inhabited by man. If early man could obtain better material there than elsewhere, as was probably the case, he would naturally have manufactured his implements there. Most or all these implements are found near the edge of the plateau at no great distance from the valley, and even under the present rigorous desert conditions I have frequently found Fellahin from the valley up to thirty and forty miles inland, remaining for days away from the habitable cultivation working small veins of rock-salt or gathering bats' dung in caves. In exactly the same way may early man have gone into the desert fringe to obtain and work his flint. Again, do the "valley erosion and waterfalls" prove a greater rainfall in the time of man? It seems to me that the greater part of the wadis may very well have been carved out before man appeared on the scene; while it is doubtful if there was ever much erosion of the valley by the Nile, the main part of the gorge probably having been formed in Pliocene times largely by earth-movements. If the plateau was really vegetated and habitable, how is it that no traces of man are found at any distance from the Nile Valley? I have crossed the Libyan desert and traversed a good deal of the Eastern plateau, but never met with any remains of early man or anything to suggest that either plateau might have been habitable even in very remote periods.

I do not pronounce either for or against Palæolithic man in Egypt,

[1] Archæological Report of the Egypt Exploration Fund, 1896–7, p. 48.
[2] Petrie & Quibell: "Naqada and Ballas," p. 49.

but I do say that so far both evidence for and against has been unsound. We must carefully collect and examine all available facts both archæological and geological, as only by a combination of both can the question of the age of different groups of flints and the presence or absence of Palæolithic man be determined.

IV.—On the Constitution, Origin and Dehydration of Laterite.

By T. H. Holland, A.R.C.S., F.G.S., Geological Survey of India.

I. Introduction.

A DESCRIPTION by Professor Max Bauer [1] of rock-specimens collected in the Seychelle Islands includes a special discussion of the laterite, which, he shows, agrees in essential characters with bauxite. As bauxite is the most valuable source of aluminium, and its origin has hitherto been without a completely satisfactory explanation, this work by Bauer opens a question of double interest to the owners of land in the humid parts of the tropics.

As with many such interesting questions, more than one worker has blundered near the correct solution without feeling his convictions sufficiently mature for publication.[2]

Apart from priority secured by publication, I shall not be departing far from justice to others in giving Dr. H. Warth, then of the Geological Survey of India, the credit of having first, or at any rate independently, suspected laterite and bauxite to be essentially similar. In 1893 Dr. Warth found a specimen in the Palni Hills, Madras Presidency, which so strongly reminded him of bauxite that he had an analysis made, and this showed over 60 per cent. of alumina ($Al_2 O_3$). The analysis did not, however, show the constitution of the laterite, and did not distinguish between the alumina combined with silica and that present as a simple hydrate. For various reasons, constituting a story in itself, this question had to give place to others, and was merely noted by Dr. Warth and myself as one to be kept in mind.

During the four years following, my observations in Peninsular India gradually led me to suspect that kaolin and many other hydrated minerals, generally regarded as weathering products, have not originated by the action of subaerial agents, but some, if not all, of them are produced by vapours from subterranean sources, whilst, on account of its peculiar distribution, serpentine must remain under the suspicion of being due to submarine action until its mode of occurrence in India can be explained on other grounds.[3] Kaolin

[1] Bauer, "Beiträge zur Geologie der Seychellen": Neues Jahrb. für Min., etc., 1898, vol. ii, p. 163.

[2] Cf. Henatsch, "Ueber Bauxite und ihre Verarbeitung": Inaug. Dissertation, Breslau, 1879. Mr. F. R. Mallet, to whom I am indebted for reading the proofs of this note during my absence from England, approached dangerously near comparing laterite with bauxite when he pointed out in 1881 that the ferruginous beds of Ulster, associated with bauxite, resembled the laterite on the Deccan Trap in India. ("On the ferruginous beds associated with basaltic rocks of North-Eastern Ulster, in relation to Indian laterite": Rec. Geol. Surv. Ind., vol. xiv, p. 139.)

[3] Holland, "A Contribution to the Discussion on Rock-Weathering and Serpentinization": Geol. Mag., 1899, Dec. IV, Vol. VI, p. 540.

has, of course, been frequently referred to as a product of solfataric action, and the evidences have recently received exhaustive treatment by H. Rösler, who, without referring to Bauer's work, doubts Glinka's suggestion that kaolin and laterite are mere climatic facies of weathering products.[1]

The frequent occurrence of kaolin as an undoubted product of what Judd defined as solfataric action,[2] and the absence of evidence to show that it was formed by the action of subaerial agents, naturally suggested that our laterites, generally spoken of as ferruginous clays, should be more critically examined. Dr. Warth's suggestion naturally occurred to me, and steps were taken to investigate the chemical constitution of laterite; but before any real progress in the work had been made, Dr. Bauer's paper appeared, and, as I think, practically settled the question. What is true of the Seychelle laterite must, so far as one can judge, be true also of the laterites in India which have not been sifted by running water. To this point and a speculation I will return after briefly summarizing Bauer's results.

The following two analyses by Professor Busz are employed in Bauer's discussion :—

		I.		II.
SiO_2	...	52·06	...	3·88
Al_2O_3	...	29·49	...	49·89
Fe_2O_3	...	4·64	...	20·11
CaO	...	trace	...	—
H_2O	...	14·40	...	25·98
		100·59		99·86

No. I is a laterite capping granite, whilst No. II was found on, and was presumably derived from, diorite on the Seychelle Islands. Bauer says that the treatment of the first specimen with boiling hydrochloric acid leaves about 50 per cent. of fine quartz-sand, which can also be detected when the laterite is moulded between the fingers. Similar treatment of No. II leaves a correspondingly small residue of quartz. He, therefore, assumes that the silica is present as quartz mechanically mixed with the other constituents; thus, by removing the silica, and calculating the results to 100, we obtain :—

	I.			II.		
	Per cent.		Mol. ratio.	Per cent.		Mol. ratio.
Al_2O_3	... 60·68	...	0·36	51·98	...	0·34
Fe_2O_3	... 9·56	...	0·04	20·95	...	0·10
H_2O	... 29·76	...	1·00	27·07	...	1·00
	100·00			100·00		

In both of these cases the molecular ratio of $Al_2O_3 : H_2O$ approaches that of gibbsite, $Al_2O_3 . 3\,H_2O$; but even without considering the water united with the ferric oxide it is still deficient,

[1] Rösler, "Beiträge zur Kenntniss einiger Kaolinlagerstätten": Neues Jahrb. für Min., 1902, vol. xv, Beilage-Band, pp. 231–393.

[2] Judd: Quart. Journ. Geol. Soc., 1890, vol. xlvi, pp. 341–382.

showing that with the gibbsite there is a certain amount of a lower hydrate, probably diaspore, $Al_2O_3 . H_2O$.

Microscopic examination shows that the ferric hydrate occurs as a casual staining between the fine scales of gibbsite, the two being mechanically mixed; and as the iron-staining varies in colour from yellow, through brown to dark red, the hydrate varies in composition, including in different specimens, or at different stages of dehydration in the same specimen, the various imperfectly defined forms of ferric hydrate which have been given specific names—limnite, $Fe_2O_3 . 3H_2O$; xanthosiderite, $Fe_2O_3 . 2H_2O$; limonite, $2Fe_2O_3 . 3H_2O$; göthite, $Fe_2O_3 . H_2O$; and turgite, $2Fe_2O_3 . H_2O$.

The results obtained by the analysis of a 'laterite sand'—the so-called 'low-level' or detrital laterite of Indian geologists—show a still smaller proportion of water, indicating a larger proportion of diaspore and probably of the less hydrated forms of ferric oxide. In this case, also, there is an admixture of quartz-grains, some, as they ought to be, partly rounded.

Now then, there appears to be no doubt that during the weathering of aluminous silicates in the tropics, the silica, alkalies, and alkaline earths are removed in solution, whilst the alumina and ferric oxide become hydrated and remain behind mechanically mixed, often as pseudomorphs of the original mineral structures of the rock. In microscopic structure and in chemical constitution Professor Bauer has now shown that laterite reproduces the essential characters of bauxite. As laterite is so abundant in tropical, moist climates, its occurrence offers a simple explanation for the origin of bauxite, and is merely another expression of the results of the prevalence of tropical conditions in some higher latitudes during past geological ages. With fair proof of the fact that the aluminous base of laterite is a hydrate and not a hydrous silicate, several questions naturally present themselves: to call attention to some of these is the object of this paper.

II. Possible Organic Origin of Laterite.

The Seychelle specimens do not tell the whole story; for there are laterites *and* laterites. In India we distinguish between 'high-level' and 'low-level' laterites: the former are lying on the rock-masses from which they have been formed without removal from the place of origin. Amongst these we have lateritic caps below elevations of about 5,000 feet in the Peninsula of India, with marked concretionary structures, whilst at higher levels the decomposition-products show less pronounced signs of concretionary structures, and often retain perfect casts of the original structures of the rock from which they have been derived. 'Low-level' laterites have a detrital origin, and consist of sand-grains in which quartz as usual predominates; they merely differ from other detrital rocks in the abundance of lateritic cement. In this form, which is our most abundant type, and the one consequently most taken in the regretfully few analyses which have been made, there is a much smaller quantity of alumina and a higher percentage of iron. This

is due probably to mechanical sorting by running water, the light, scaly gibbsite being carried away, whilst the heavier ferric hydrate remains with the grains of sand. Thus, taught by the natives who sometimes use it as an iron-ore, we have been blinded to the true nature of laterite by keeping an eye on its possible use as a source of iron. That the Seychelle specimens are of the 'high-level' type seems probable from the fact that the ratio between alumina and ferric oxide in each case corresponds approximately with that of the rocks on which they lie, granite in one case and diorite in another. They retain, too, just as much silica as one would naturally expect to occur as free quartz in the original rocks.

With laterite one ought also to consider the material known generally as *kunkar*, without much respect to the original meaning of the Hindustani word. Laterite and *kunkar* appear to be formed by precisely the same process; but they differ from one another in composition, and the reason for this is seen in South-West India. The Western Ghâts of Malabar divide areas which differ greatly in rainfall. The south-west monsoon, breaking against the hills of Travancore and Malabar, precipitates between 100 and 200 inches of rain on the western slopes during the wet season; whilst on the eastern side the plains of Coimbatore receive no more than about 20 inches of rain during the year, and the field-worker in Coimbatore sees with satisfaction the great bank of cloud spending its fury on the Ghât ranges, whilst a cool breeze blows through the Palghât gap, and spreads itself over the eastern plains with seldom more than a sprinkling of rain. As we pass from the west to the east side of the western ridge, there is a complete change in the weathering products. On the western side, where running water is abundant, there is the crust of typical ferruginous laterite. On the east side, where the climate is quite as moist, but actual precipitation of water very limited, the ferruginous character is masked by the great predominance of lime-carbonate, and the material, from its resemblance in composition to the nodular bodies in the Gangetic alluvium, is known to the Public Works officer as *kunkar*, being similarly used as a source of hydraulic cement. In *kunkar* we have the *total* products of decomposition; exposure of this to running water would remove the lime, and leave a residue indistinguishable from laterite. Both substances thus originate in the same way by the decomposition of rocks in a tropical, moist climate.

Laterite is characteristic of the tropical belt, and so, too, is *kunkar*; but this possibly is merely a case of intensity: both may be formed to a less noticeable degree in temperate regions. Still, in their typical forms these peculiar substances are eminently characteristic of the tropics — one of a wet and the other of a humid climate.

That the formation of laterite is a mere question of average temperature seems unlikely, for lateritization is very prominent at elevations of 6,000–7,000 feet in the Nilgiri and Palni Hills in South India, where the temperature varies very little above or below 60° F. The concretionary structure so characteristic of the laterite

of lower levels is not, however, developed at this elevation, which accounts for the restrictive remark in the Manual of the Geology of India[1]; and it is likely that the high temperature assists the concretionary structure in the way indicated in the sequel. Preparatory to the suggestion offered below it should be premised that the climate of the Nilgiri and Palni highlands differs from that of the temperate zone in the absence of a real winter: in the fact that very low temperatures do not occur; so a winter temperature, maintained for a long period, may be fatal to pronounced lateritic development, and may possibly prevent its formation altogether in high latitudes, where, as we know, fresh felspars are preserved in detrital materials.

On the other hand, when vapours (or liquids) from deep-seated sources act on aluminous silicates, presumably at high temperatures, hydrous silicate of alumina, kaolin, is formed, and not, so far as recorded, the simple hydrous oxides.

Now, putting these facts together, I would suggest that we look for the explanation of laterite, not in simple chemical reactions, but in the action of some lowly organism having the power of separating the alumina, which, after the manner of many plants, it does not want, from the silica, which is necessary for its life, but which, being in a soluble form, is removed again by the alkaline solutions. Such a form of life might thrive in the moist climate of the tropics, even to a temperate altitude, but might find life intolerable in a land subject to severe winters, such as we get in the temperate zone and in North India, where we find no laterite. Such an organism would form *kunkar* as well as laterite in a moist, warm climate, the difference in the products being due merely to their after-treatment by water. For obvious reasons, too, such an organism could not live under conditions of kaolinization. But apart from the agency of life I see no chemical reason why an aluminous silicate should undergo a more complete decomposition at the comparatively low temperature of the tropics than at the high temperature of subterranean situations; the contrary, indeed, seems more natural. And if the temperature *does* so affect chemical action, we still might wonder why laterite does not occur on the foot-hills of the Himalayas, where there is an abundance of moisture and where the average annual temperature is as high as on the Nilgiris. Chemical changes which cease at low temperatures commence again as soon as the suitable physical conditions are restored, and laterite, consequently, would be expected to form in North India during the summer. But the distribution of an organism might very well be limited by the extremes of climate, when possibly the average annual temperature is not below what would be congenial to it if maintained.

If this fancy turns out to be well founded we must add lateritization to the long list of tropical diseases, against which even the very rocks are not safe. But it is a big step between the establishment of a reasonable suspicion and the actual detection of the

[1] 2nd ed., 1893, p. 375.

bacillus at work. There may be many forms of life taking advantage of the soft, moist, lateritic medium, but it will not be an easy matter to convict, amongst these, such as may take an active part in breaking up the aluminous silicates.

It is hard to believe that the few degrees by which a tropical exceeds a temperate climate is sufficient to so strikingly increase the chemical activity of the weak organic acids percolating through the soil. But that such a small difference of temperature affects low forms of life is painfully evident to those who have to maintain the daily fight of life in the tropics. The enormous depths to which the complete destruction of rocks extends in moist, tropical, climates has frequently been described by travellers; but no description reproduces the actual impression caused by the deep cuttings in places like the hill-roads of South India, where, on a newly-cut face the minute structures of the gneisses are perfectly preserved in material as soft as putty. And yet, as I have pointed out elsewhere,[1] there is a sharp and sudden passage from the soft decomposition-product to a rock so fresh that not a sign of alteration can be detected in it under the microscope. This is true, too, of boulders isolated from the main mass of rock and embedded in the decomposition-product: within the distance of an inch one passes from the soft material, which we have unwittingly called clay, to ideally fresh, water-clear felspars.

If this alteration were due to the mere infiltration of water through the rocks one would expect to find signs of incipient alteration to greater depths in the solid rock. But if it is due, as I suspect, to a growth at the surface, the sharp line of junction is no more than we should expect. Darwin, against whose observation nothing was secure, and who never observed without deduction, thought the thick decomposition mantle on the Brazilian coast was due to hydration under submarine conditions.[2] But *that*, we are certain, is not the case with the South Indian hill-ranges; the action is purely subaerial, and the sea has never been near the rocks.

But the existence of low forms of life capable of decomposing aluminous silicates would be no surprising discovery after what we know of the action of the so-called nitrifying bacteria in converting inorganic salts into proteids for the use of higher plants; of those which, contrariwise, break up nitrogenous organic matter to form nitrates; of the peculiar anaërobic forms which break up sulphates and separate the sulphur, and of the aërobic forms which convert sulphides into sulphates.

Amongst the forms almost certain to occur in newly-forming laterite will be relatives of the remarkable genera *Crenothrix*, *Cladothrix*, etc., which are active agents in the oxidation of ferrous salts, depositing the insoluble form of ferric oxide in their cell-walls as a part, apparently, of the physiological activity of the organism. There are probably bacteria akin to these which assist in the

[1] Holland, "The Charnockite Series": Mem. Geol. Surv. Ind., 1900, vol. xxviii, p. 197.

[2] Voyage of the "Beagle," 2nd ed., 1876, p. 427.

oxidation of the ferrous salts released during the lateritization of basalt. But the important forms to identify will be those which supply the necessary energy for the more complete disintegration of an aluminous silicate than appears to be possible by chemical means alone at the temperature of the atmosphere. Our explanation cannot be accepted as wholly satisfactory until the agents have actually been identified; but there is enough circumstantial evidence to warrant the suspicion that a special form of organism is the direct cause of lateritization in the tropics.

III. The Dehydration of Laterite.

There is another interesting question suggested by laterite. The analysis by Professor Busz of the lateritic sand from the Seychelles shows a lower degree of hydration than that which characterizes the laterite formed *in situ* on the granite and diorite. This agrees exactly with our experience in India.[1] 'Low-level,' recemented laterite is always less hydrous than the 'high-level,' freshly-formed material; and there is in general a correspondence between the development of concretionary structures and the degree of dehydration. Soft, yellow masses, freshly cut out with a spade, gradually become hard and deepen in colour, until they approach a reddish-brown. Every Public Works officer knows this, and feels with satisfaction that his bridges, built with laterite, strengthen with age.

The drying of laterite is not a case of mere loss of mechanically included water: the ferric hydrate parts with combined water and passes from the condition of limonite, with its yellow colour in powder, to that of turgite, whilst at the same time a concretionary, and in places microcrystalline, structure is developed. The hydrated alumina apparently undergoes a similar change, passing from gibbsite, $Al_2O_3 . 3 H_2O$, to diaspore, $Al_2O_3 . H_2O$, and this irregular loss of water is probably the cause of variation in bauxite, which is intermediate between gibbsite and diaspore, but is inconstant in composition, as one would expect in a mixture.

Our knowledge of the constitution of ferric hydrates is very imperfect, and, of the natural forms, far less precise than one would imagine from the number of specific names employed to distinguish forms which are probably mere mixtures possessing insufficient individuality to crystallize. Göthite, corresponding to diaspore, is the only hydrate definitely crystallized. But the anhydrous oxide, hematite, on the other hand, shows an eminent tendency to crystallize, and is amongst those minerals which might be well spoken of as crystallographically robust. The tendency to crystallize is thus exhibited by the anhydrous and least hydrated members of this group.

At the risk of sacrificing precision for brevity we can refer to this tendency for the physical molecules to come together and form crystals as stronger than the chemical affinity between ferric oxide and water. The two manifestations of energy thus come into

[1] Geol. Mag., December, 1899, p. 542, note 1.

competition, and the loosely held second and third molecules of water are ejected to permit the crystallization of göthite, $Fe_2O_3 . H_2O$, or possibly even the whole of the water to form hematite, Fe_2O_3. It must be this action which develops the crust of turgite on nodular masses of limonite; for turgite itself has no individuality and is probably only an intimate mixture of anhydrous and hydrous oxides.

In one sense the change from gibbsite to diaspore is analogous to the dehydration of ferric hydrate. A comparison of the molecular volumes of corundum, Al_2O_3; diaspore, $Al_2O_3 . H_2O$; and gibbsite, $Al_2O_3 . 3 H_2O$, indicates a lower molecular volume for one molecule of water in diaspore than for each of the three molecules in gibbsite. The figures are as follows :—

$$(v)\ Al_2O_3 . H_2O - (v)\ Al_2O_3 = 35{\cdot}0 - 25{\cdot}5 = 9{\cdot}5 \quad . \quad . \quad . \quad . \quad (a)$$
$$(v)\ Al_2O_3 . 3 H_2O - (v)\ Al_2O_3 . H_2O = 66{\cdot}5 - 35{\cdot}0 = 31{\cdot}5 \quad . \quad (b)$$
$$(v)\ Al_2O_3 . 3 H_2O - (v)\ Al_2O_3 = 66{\cdot}5 - 25{\cdot}5 = 41{\cdot}0 \quad . \quad . \quad . \quad (c)$$

Of these three equations, (*a*) shows the volume of one molecule of water in diaspore to be 9·5, which is identical with the result obtained by deducting (*b*) from (*c*), whilst (*b*) gives the volume of the second and third molecules of water in gibbsite to be together 31·5, or an average of 15·75.[1] Thus, the constitution of gibbsite is more correctly expressed by the formula $Al_2O_3 . H_2O + 2 H_2O$. Now, the relation between the first molecule and the other two is precisely analogous to the relation between Graham's 'water of halhydration' and the 'water of crystallization' in soluble salts. Thorpe and Watts have shown—with sulphates as examples—that the first molecule has a volume near 10, whilst the water of crystallization may be as much as 16. They have also confirmed the conclusions of Graham, and extended by Favre and Valson, to the effect that more heat is evolved in the act of combination of the first molecule—that in which the greatest contraction occurs—than is evolved in the combination of the remaining molecules. Similarly, the water of crystallization is driven off more easily than the water of constitution or halhydration.[2]

The evidence of the molecular volumes thus indicates a willingness on the part of gibbsite to rid itself, on small provocation, of two molecules of water. This willingness is shown by its ready transformation into diaspore. In the case of limonite the tendency to dehydration is still more pronounced; for it seems from the evidence of laterite, that even a completely anhydrous condition can be attained in a tropical climate which is at the same time humid for much of the year. Whether limonite represents a chemical compound or not it is impossible to say, but gibbsite, which forms distinct crystals, is presumably a definite compound, and its loss

[1] The actual identity of results by these two equations is, of course, accidental; for it is not known that the molecular volume of Al_2O_3 remains constant in all its combinations. But, for reasons that need not be stated now, it is certain that its variations in crystallized compounds is very much less than the difference between the molecules of water.

[2] Thorpe & Watts, "On the Specific Volume of Water of Crystallization": Journ. Chem. Soc., 1880, vol. xxxvii, p. 102.

of water must consequently be a chemical change of the endothermic kind. But where, seeing the compound is not heated from extraneous sources, does the energy come from? I believe the answer to this question is given by the fact that the dehydration is accompanied by the crystallization of the molecule of ferric oxide and of the aluminic hydrate with the lowest possible molecular volumes.

The concretionary structures which characterize laterite and bauxite are but the foreshadowings of crystallization—a preliminary segregation of the molecules, accompanied by expression of the water—to which all well-defined chemical compounds aspire, though molecular movement may not always be sufficiently free to produce noticeable results at ordinary temperatures.

The comparative opacity of the iron-oxides prevents the use of the microscope as a means of tracing the passage from the most hydrated to the anhydrous condition; but it seems very likely, judging by the analogy of the aluminous hydrates, that some of the intermediate stages are mere mixtures, and that there is the usual tendency to pass into the forms with smaller molecular volumes. The tendency to crystallize—the grouping of physical molecules—is analogous to the manifestation of chemical affinity amongst chemical molecules, and there must, in the same way, be differences of intensity. I am indebted to Professor A. H. Church for references to the case of arsenious oxide, As_4O_6, which, on passing from the amorphous to the octahedral form, gives out 5,300 gram-units of heat, whilst the passage from the amorphous to the monoclinic form is attended by the production of 24,950 heat-units per As_4O_6 grams. In these cases the octahedral form has a lower molecular volume than the amorphous, and the monoclinic form lower than either. Thus, the change of physical state amongst solids may result in a change of entropy as great as that due to chemical change; that, in fact, crystallization may cause a measurable dissipation of energy.

Thomsen gives the heat of formation of aluminic hydrate $[Al^2, O^3, 3\,H^2O]$ as 388,920 calories, and of ferric hydrate $[Fe^2, O^3, 3\,H^2O]$ as 191,150 calories, both values being less than the heat of formation of the respective anhydrous oxides.[1] Without a direct determination it would be unsafe to regard the dehydration of these compounds as an exothermic change; but with the simultaneous crystallization of diaspore in one case and of göthite or even hematite in the other, one is safe in assuming that the changes manifested by the development of concretionary structures in laterite are in the total exothermic, and that the 'tendency' to crystallize is a condition worthy of serious consideration and of comparison with chemical changes.

The expression *crystalline affinity* might be employed conveniently as an analogue of chemical affinity for the tendency exhibited by substances to form crystals by the regular disposition, as we

[1] J. Thomsen: "Thermochemische Untersuchungen," 1882, vol. iii, pp. 240, 294. Roberts-Austen: *Nature*, August 8th, 1901, p. 360. Naumann: "Thermochimie," 1882, p. 514.

imagine, of physical molecules. That this tendency is variable is evident from the fact that some substances readily form well-developed crystals, whilst others require a prolongation of the physical conditions which are favourable. The display of energy due to chemical union is in general sufficiently pronounced to be noticeable; but there must also be a display of energy when physical molecules have the privilege of exhibiting crystalline affinity, and it is not unlikely that in some cases the heat of formation of a crystal may be greater than that of a chemical compound, in which case, as the hydrates of iron and alumina appear to teach, a chemical compound may be broken up to permit some constituent to crystallize. We shall probably be near the truth if we regard the crystalline affinity as the heat of formation of a double molecule. Thus the thermal value of $[Fe^2 O^3, Fe^2 O^3]$ may exceed $[Fe^2 O^3, 3 H^2 O]$, in which case, under suitable physical conditions, the water would be driven out of limonite and a crystal of hematite formed, because the system is one containing potential energy due to the proximity of molecules of $Fe_2 O_3$. Similarly, the potential energy of a system in which $Al_2 O_3 . 3 H_2 O$ exists contains sufficient potential energy to displace the two molecules of water having a high specific volume to form a crystal of the compound $Al_2 O_3 . H_2 O$. In other words, the thermal value of $[Al H O^2, Al H O^2]$ is greater than $[Al H O^2, H^2 O]$, and the formation of diaspore at the expense of gibbsite is an exothermic change capable of spontaneous development under suitable conditions.

At a high temperature diaspore would change further to corundum, a change equivalent to the formation of hematite at lower, probably at ordinary, temperatures. Aluminic oxide thus shows a greater affinity for water than ferric oxide does, which is shown by the facts, (1) that limonite is not found crystallized, whilst gibbsite forms definite crystals, and (2) that gibbsite at ordinary temperatures is reduced to diaspore as an exothermic change, whilst the anhydrous compound, hematite, is formed from ferric hydrate under similar circumstances.

One is aware, of course, that we have but a vague idea of what the amorphous state means. But the passage to the crystalline condition is in general one accompanied by a decrease in molecular volume; and with the striking case of arsenious oxide before us, it would be well to examine more critically cases in which the manifestation of energy is less pronounced, though necessarily existent and possibly measurable.

IV. Summary.

The objects of this note are three: (1) to call attention to the essential chemical similarity between laterite and bauxite; (2) to offer a theory for the formation of laterite in the moist tropics; (3) to suggest an explanation for its spontaneous dehydration.

(1) Laterite has generally been referred to as a ferruginous clay; but if the term clay is restricted to substances having a basis of hydrous silicate of alumina, this definition is incorrect. The

alumina in laterite exists, as it does in bauxite, in the form of hydrous oxides. Kaolin must thus be removed finally from the list of weathering products; it is formed generally, perhaps exclusively, by the action of subterranean vapours on aluminous silicates. This conclusion necessitates a re-examination of many so-called argillaceous substances; many doubtless include the kaolin already existing in the kaolinized aluminous silicates before they are exposed to the weather. But it is probable that some of the red clays of past geological ages, formed under subaerial conditions, contain free hydrous oxides of alumina; and for those that are shown to contain hydrous silicate of alumina, it would be well to test the possibility of a secondary reunion of aluminic hydrate and free silicic acid.

(2) To account for the fact that an aluminous silicate undergoes a more complete disintegration under tropical conditions than under the deep-seated and presumably high-temperature conditions of kaolinization, the writer suggests that laterite is due to the agency of lowly organisms, possibly akin to the so-called nitrifying bacteria. With these there are probably forms akin to the bacteria which oxidize and fix ferrous compounds, and which, precipitating the silica in the colloid form, permit its removal by the dilute alkaline solutions simultaneously formed. This would account for the facts, (*a*) that laterite is confined to the tropics, or at least is more conspicuously developed under tropical conditions; (*b*) that although the laterite cover is 100 feet or more in thickness, there is a sharp change from the soft decomposition-product to the absolutely fresh rock below; (*c*) that though laterite can form at temperate altitudes, it is not observed in temperate latitudes, where, with a similar average annual temperature, there is a prolonged winter; and (*d*) that laterite is a superficial product.

(3) The development of concretionary structures in laterite being accompanied by the loss of water, it is suggested that, in compounds where a constituent is loosely held, the 'crystalline affinity' by which physical molecules tend to unite and form crystals may be more energetic than the chemical affinity; that, in other words, the heat of formation of a crystal of Fe_2O_3 may be greater than the thermal value of the compound [Fe^2O^3, $3H^2O$]. Hence crystalline hematite may form spontaneously at the expense of amorphous limonite. Similarly, the two molecules of water having a high specific volume in gibbsite ($Al_2H_2O_4 . 2H_2O$) are expelled by the formation of a crystal of diaspore ($Al_2H_2O_4$). Assuming that the change of entropy can be expressed in thermal formulæ, the value [$AlHO^2$, $AlHO^2$] must be regarded as greater than [$AlHO^2$, H^2O], and both limonite and gibbsite must be regarded as unstable at tropical temperatures when both exist together. As the ferric hydrate shows a greater tendency towards dehydration than the corresponding hydrate of alumina, it possibly acts as a catalytic agent; and it would be unsafe, therefore, to assume, from the evidence of laterite, that *pure* gibbsite possesses the power of spontaneous dehydration.

V.—The Permanence of River Valleys.

By the Rev. E. Hill, F.G.S.

A GOOD deal has been written on the permanence of ocean basins, but about the permanence of river valleys I have never noticed any discussion. The causes which tend to maintain the existence of a river valley are indeed obvious and simple; probably this simplicity and obviousness are the very reasons why so little has been said. Yet even if superfluous it may be interesting to bring together some of these causes, such at least as have come into my mind.

What actions can even temporarily obliterate a channel cut into the surface of the land? Submergence followed by re-elevation: what will that effect? During submergence sediment may be deposited. If this be deposited as a mantle of equal thickness over the whole bottom, the surface on re-elevation would have the same shape as before; hills and valleys would reappear of equal relative heights and depths. Probably, however, the amount of sediment would be greater in greater depths: more would be deposited in valleys than over hills. Then on re-elevation the surface would appear with features smoothed down, but not obliterated. Where a valley had been, a valley would again be; less deep, but with a fall in the same direction, tributaries in the same position, and the same catchment basin as before. Rain and rivers would resume their old courses and their old work; the valleys would be the same as before. Imagine even the most improbable case of a sedimentation such as should bury hills and valleys alike; let every feature vanish as a path is hidden beneath a sheet of snow; even then the valley would reassert itself. For the sediment would be moist, and on re-elevation would dry and shrink. Shrinkage would be most where depth was greatest: that would be where the valley had been. Thus shrinkage would restore the outlines of the old drainage system; water would follow the same courses as before; each valley would reappear and its excavation begin anew.

If obliteration be effected subaerially there arises a different state of things. Messrs. Anderson and Flett describe[1] the Souffriere eruptions of 1902 in St. Vincent as having with their ashes filled up the stream valleys level with their banks: "those who visited the country shortly after the first eruption describe it as having a smooth, gently rolling surface like that of blown sand." Blown sand itself or steppe dust may obliterate channels. One may imagine a complicated river system thus replaced by an almost featureless surface of æolian loess. Yet even in this case the original relief of the ground will surely seldom fail to influence its subsequent shape. In the paper cited above it is said[2] that after rain had fallen "the knife-edges between the valleys were the only parts retaining the original smooth surface . . . already many of the streams have thoroughly cleaned out the ash from the upper

[1] Royal Society Proceedings, vol. lxx, p. 432.
[2] Ibid., p. 435.

part of their channels . . . the valleys at first almost obliterated are now reassuming their old appearance."

A similar effect may be produced in a different manner.[1] Such superincumbent drift will generally be porous, and more porous than the lost land-surface. Water sinking through this porous drift will sink to that old surface and find its way along that to the old hidden channels. Then flowing along these it will tend to erode subterraneously, and by the consequent land-sinkings it may produce a system of surface channels which will follow the lines of the former drainage system. I suppose the subsidences in the Cheshire salt-working district indicate hidden hollows of Triassic times: similar causes may lead to similar results.

Man makes reservoirs by dams across valleys; so Nature with landslips, moraines, lava-flows. When the water has risen above its dam, the river will commence to cut a channel through, and will resume its previous course. Sometimes, however, it is able to overflow across a lateral col: then the whole drainage system is altered; a lake replaces the old valley, and this begins to be silted up. Yet even now the valley may not be obliterated for ever. Allow sufficient time, the new channel will be cut down to the lake-floor, the lake will be empty: its bed will now seem a pair of opposite valleys meeting at a common outlet. What was depression before will be depression still. The result will be a physical feature such as we now see on the south-east side of the Mont Blanc chain. A vast moat extends below that mighty wall, from the Col de Ferret to the Col de Seigne, collects the whole drainage of the south-east face, and pours it out, opposite the central pass of the Col du Géant, through the sluice-like cleft where Courmayer stands. I do not affirm that the Allée Blanche was so produced; but an uprising of the Col de Seigne would have been able to produce it.

A lava stream down a valley would produce most serious alteration, even if it did not completely fill the trough, for the materials of the cast would be more resistant than those of the mould. Sometimes, indeed, erosion might go on along its edge, and create a new valley roughly parallel to the old. But lava-flows are exceptional agencies.

The depressions and re-elevations hitherto considered have been regarded as simple drops or lifts without tilt. It was convenient to keep separate the effects of deposition and the effects of altered inclination. Similarly, in considering the consequences of tilts, let us separate those which only alter the fall of the valley-bed from those transverse to them which alter the slope of the valley-sides. The former, if they increase the fall of the river, will increase erosion and deepen the valley; if they decrease it, will check deepening, may cause deposition to begin: they may reverse the flow, with erosion continued: they may create lakes, which may fill up parts of the valley. But very rarely can a valley by this means be so obliterated that every trace is lost.

[1] Suggested by a friend in conversation.

Far different is the power of a tilt transverse to the stream-line. This will make one side of the valley more steep, the other less, and may thus go on to some extent without destroying the water-flow. But so soon as the angle of tilt exceeds the slope of the opposite valley-side, that slope becomes reversed; instead of shedding water down to the river-bed, it drains water away from it, and the valley is a valley no more. Its place in the scenery of the country is now taken by a feature which shows as a steeper slope with a gentler slope below: in profile like a cliff above a talus, or a river wall running along a river beach. But since a valley is rarely a straight trench, and a tilt can seldom be uniform, different parts will suffer in different degrees, so that traces of the trench may frequently remain. Those valleys will suffer most which are most nearly strike-valleys to the dip of the tilt. I suppose it has been already pointed out somewhere that, in a sinuous valley, the first effect of a tilt transverse to its general direction will be the creation of lakes on the lower sides of its windings.

I conclude, then, that the agents most able to efface a river valley are those which change the inclinations of strata; and that even these agents may effect considerable alteration in the surface without effecting a valley's destruction. But a powerful and incessant enemy is the river itself. For this in its sinuous lower course is ever tending to widen its trough, that is, to make the valley a less conspicuous feature of the relief. The valley opens out into an estuary; the estuary into a gulf, perhaps a sea.

'Permanence' does not precisely express the property which has here been discussed. This somewhat resembles a power of recovery from disaster, of awakening from hybernation or from sleep; an animal's reproduction of a lost limb; the regrowth of a broken crystal restoring its form. The valley shape may be lost for a while, but there remains a possibility of its restoration. We might almost talk of its vitality.

The valley retains its vitality, though its own river be burying it with sediment. Such sediment comes from the ridges between channels; the channels may be filling up while the ridges are being worn down, but if the channels are being cut, so also in general the ridges. Thus ridges may be the localities of more continuous degradation, and of irreparable destruction.

In view of this inherent vitality we may well believe some present valleys to be extremely ancient features of our earth's surface.

VI.—On the Original Form of Sedimentary Deposits.[1]

By the Rev. J. F. Blake, M.A., F.G.S.

(*Concluded from the January Number, p.* 18.)

WE may now examine how far these theoretical conclusions explain and are confirmed by what is seen in nature. First we know that in most formations there are great masses of what is now, or must have been once, fine-grained sediment. These often

[1] A paper read at the Meeting of the British Association, Belfast, September, 1902.

make up the bulk of the formation. We may quote the Ordovician, Silurian, and Devonian slates, the Keuper, Lias, Oxford and Kimmeridge Clays, the Gault and London Clay; but we can give no such list of thick masses of marine sandstone. Fine sediment, therefore, has, as a matter of fact, made thicker masses of rock than coarse sediment; but this could not be the case if deposits thinned out seawards, where the fine sediment is carried. Again, it is impossible to imagine a thickness of a thousand feet and more constantly occupying a position near the shore; there is no room for it. If you depress the land, you remove the shore. To draw a diagram of one such deposit, an unnaturally scooped-out shore has to be assumed, and it is impossible to repeat the process. Indeed, a shape such as Fig. 2 is the only one possible for an indefinite number of deposits on a sloping sea-bottom. There is nothing, of course, to prevent a thinning out *beyond* the maximum, and a film from most terrigenous deposits does, no doubt, spread in that direction.

And if we go into details we find that where a thick deposit of fine sediment thins, we get independent evidence of an approach to a shore, but none where it remains thick.

Take the Cambrian and Ordovician strata as a whole. It has been found necessary by those who assume that a thickening of strata indicates approach to a source of supply to postulate a large continent occupying the position of the north part of the present Atlantic Ocean during those early times. But Mr. C. D. Walcott, the Director of the U.S. Geological Survey, writing on "The North American Fauna during Cambrian Time,"[1] states that "There is certainly no evidence to show that since the beginning of Palæozoic time the beds of the deeper seas have been elevated above the surface of the waters"; also that "The interior continental area [of North America] was outlined then ["at the beginning of Cambrian time"], and it has not changed materially since," and he draws a diagram accordingly, showing the Cambrian strata thickening as they leave the shores of this Pre-Cambrian continent.

On this side of the Atlantic we learn from C. Schmidt[2] that in the Baltic provinces the whole of the Ordovician strata are represented by a thickness of 420 feet, whereas the same strata in this country are said to exceed 12,000 feet. Much of the latter may be of volcanic origin, but half the thickness would serve the present purpose. Now, speaking of the Scandinavian Ordovicians on the opposite side only of the Baltic, J. E. Marr[3] says: "Many of them [the fossils] are found in shallow water beds, deposited at periods when the greater part of the North-Western European area was covered with deep water"; and the fossils are certainly more abundant than in this country.

But these comparisons are on too large a scale. Let us take the case of a deposit laid down in a single basin and during a more

[1] U.S. Geol. Survey Ann. Rep., vol. xii, p. 563.
[2] Q.J.G.S., vol. xxxviii, p. 513.
[3] Q.J.G.S., vol. xxxviii, p. 513.

closely defined period, such as the Upper Llandovery beds of Wales and the Shropshire area. We find that in the Malvern area the May Hill Sandstone is described as entirely conglomerate and sandstone, and of a thickness of at most 800 feet. At Rhayader, according to Mr. H. Lapworth,[1] the same group contains 200–300 feet of blue shales amongst the conglomerates, and 250 feet more above them. Thus, while the whole deposits have increased from 800 to 1,100 feet, the fine-grained detrital portion has increased from *nil* to 500 feet. Still farther west Mr. Walter Keeping,[2] who allowed as much as possible for folding, could not estimate his metalliferous slate group at less than 2,000 feet, and in this group he says that occurrences of grit, even of 2–6 inches thick, are very rare. His general conclusion is, "that whilst in . . . . the dawn of the Silurian era the elevatory forces lifted the sea bed to form a land surface over the west of England and the Welsh borders, these forces influenced the greater part of Wales only in a less degree," and did "not interfere with the continuous deposit of sediment." In other words, he recognizes that these deposits are thickest, independently of their character, at the greater distance from land.

Take, again, the case of the Permians on the east side of the Pennine axis. They were deposited after the elevation of this axis, and towards the south end of their range there are indications of the shore-line both in the basal breccias and in the Magnesian Limestone. Near this shore-line the three members there found have a total thickness of 80–160 feet; but thirty miles away in the Searle boring the same beds (excluding higher ones there represented) have increased to 360 feet, to which increase the Magnesian Limestone makes only a negative contribution.

More interest in this regard attaches to the Jurassic rocks, as their thickness and character is more widely observable, and also because it was to these that Professor Hull appealed, on the assumption (for no shore-line was proved) that their "attenuation is due to the increase of distance from the sources of supply," to prove that their "sedimentary materials . . . . have come from land occupying the region of the North Atlantic."[3]

The borings that have been made at Mickleton, Burford Synett, Wytham, and Turnford have certainly demonstrated the thinning out of all the Jurassic series and the Keuper towards the south and east; and as the slope thus produced is not greater than the angle of rest of the materials, it is competent for anyone to say that all the beds are in the position in which they were laid down (vertical elevation of the whole excepted), and that the sea bed beyond them to the east was too far removed from land to receive any of these terrigenous deposits. This, indeed, is what *must* be said if deposits thin out gradually seawards. But in this case we throw over the whole argument for a 'Palæozoic ridge,' and leave the fresh-water Purbecks of Buckinghamshire wholly unaccounted for.

[1] Q.J.G.S., vol. lvi, pp. 69–135.
[2] Q.J.G.S., vol. xxxvii, pp. 141–150.
[3] Q.J.G.S., vol. xvi, p. 80.

But if, with Godwin-Austen, we account for the absence of Jurassic rocks over a wide area east of London, not by the area being too far from land, but by its being "dry land at this period," then the western side has been subsequently elevated, and the slopes which now are south-easterly were once north-westerly. We also have a proof that thicker beds as a whole correspond to deeper water, and that the original form of a sedimentary deposit as here described is the true one.

It is interesting to note what a difference in questions of palæogeography this interpretation of the form of a deposit makes. If we take Professor Hull's view, perhaps the most commonly accepted one, we read this general conclusion: "During the deposition of the Upper Palæozoic and Lower Mesozoic rocks an extensive tract of land existed to the north-west of the British Isles which afforded the materials of which these rocks are composed."[1] If, on the other hand, we take the view here put forward, apparently assumed but not definitely enunciated by Godwin-Austen, we are led to the conclusion that "The Oolites of Yorkshire and Lincolnshire were dependent on a land which lay to the east,"[2] and to the extension of this land on the map as far as the line of the North Downs.[3]

We may now test the supposed form of a sedimentary deposit by its effect upon the submarine contours. We have seen that this form involved the occurrence of a markedly rapid slope in the sea-bottom, corresponding to the seaward termination of the deposits. This slope might be at various distances from land, and affect various contour-lines, but more especially that of 100 fathoms.

In the case of the banks of coarser deposit in shallow depths, it is easy to see examples in the edges of the parallel roads of Glenroy, or in the undisturbed banks in dried-up lakes; and Godwin-Austen[4] states from actual survey that in the English Channel "the termination of deposits of a well-defined character, such as the shell-gravel beds or those of clean sand, is often by slopes more or less steep." Such banks and slopes, however, from being comparatively small and in shallow water, are easily altered by or confounded with the results of transverse currents.

It is, however, with the more extensive slopes which may mark the limit of deposits formed from sediment in suspension that we are most concerned. Possibly Godwin-Austen's observations apply to some of these deposits, as he compares them with escarpments of such when raised above sea-level, but in any case the great slope now referred to as the 'continental slope' has been known (*fide* G.-A.)

[1] Hull: op. cit., p. 8.

[2] Godwin-Austen: Q.J.G.S., vol. xii, p. 64.

[3] Since this paper was read at Belfast, I have received the 21st Annual Report of the U.S. Geol. Survey, with an account of the Cretaceous formation of Texas by R. T. Hill. A full account is therein given of the thickening seaward of the Glen Rose formation (see pp. 138–9 and 369–382). This has been preserved from denudation by the overlying Fredericksburg Division, and is bounded below by the Palæozoic rocks. Its thickening is observed in artesian wells. It has thus retained its original form, which is 'wedge-shaped,' but the *termination* seaward I cannot find described.

[4] Q.J.G.S., vol. vi, p. 79.

since 1828 in its range between Cape Finisterre and the Lizard. Absence of anything that can represent the theoretical rapid slope cannot, therefore, be brought against the theory, but it is another thing to show that this actual slope has anything to do with the theoretical. This is what Godwin-Austen writes on this subject:[1] "Of the true nature of such a sudden line of depression we can at present only form conjectures. It may represent lines of old escarpments; or, should lines of sea-cliff have gone down rapidly into sea-water where no mechanical action could modify them, such features would be preserved; lines of faults and upheaval would also present such unequal soundings; but the outline is too irregular to represent the termination of the sedimentary mass of the present seas, besides which we have constant indications of a surface of bare rock." On this it may be remarked that the interpretation recently put upon this slope by Professor Hull, following the lead of Professor J. W. Spencer in America, had already been suggested as a possible one by the writer here quoted. We note also that the possibility of its representing the termination of deposits was admitted; but this interpretation was rejected for local reasons.

These reasons require an answer at once. The particular district to which the remarks of Godwin-Austen refer is one in which the result of sedimentation is masked by the irregularity of the previous sea-bottom. It is where the submerged continuations of Brittany and Cornwall project above the general submarine surface. Both countries are terminated with bosses of granite, and this feature is continued first in the islands of Ushant and Scilly, and then by a pair of banks on which granite fragments are found, hence the irregularity and the bare rocks in the region within the 100 fathom line.

But if we make comparisons in less exceptional localities we find remarkable confirmations of the idea that the slope is due to the termination of deposits. There is here copied (by his kind permission) a portion of the map[2] prepared by Mr. Hudleston for his paper "On the Eastern Margin of the North Atlantic Basin," which has the advantage of not being drawn for the purpose of proving the present theory (Fig. 3). Compare the lines marked *a–a* and *b–b*. The first is drawn directly outwards from the south-west of Ireland, south of the Shannon. Here there are no great detritus-bearing rivers, and the contour-lines are spaced with remarkable uniformity. The second is drawn opposite the opening of St. George's Channel, which may be considered as a large tidal estuary bearing much detritus seaward. Here the 100, 500, and 1,000 fathom contour-lines are much bulged outwards, while the spaces between the 1000–1500 and 1500–2000 lines are reduced to a minimum. Thus, where there is no deposit the continental edge is scarcely marked at all, and where we may suppose that there is much deposit taking place the slope is very sharp indeed.

[1] Q.J.G.S., vol. vi, p. 86.

[2] Geol. Mag., 1899, Dec. IV, Vol. VI, Pl. III.

Again, compare the lines *c–c* and *d–d*. The first is drawn opposite the mouth of the Loire. Here the space within the 100 fathom line is very broad, while the other four lines down to 2,000 fathoms are crowded together into a quarter of the space. The second is drawn outward from the Pyrenees, and the slope is naturally rapid; but the spaces between the lower contours of 1,000 and 2,000 fathoms are together *twice* as broad as along the first line. Thus the deposits along the west coast of France have caused the continental slope there to be twice as rapid, instead of half as rapid, as it should be in comparison with the north coast of Spain.

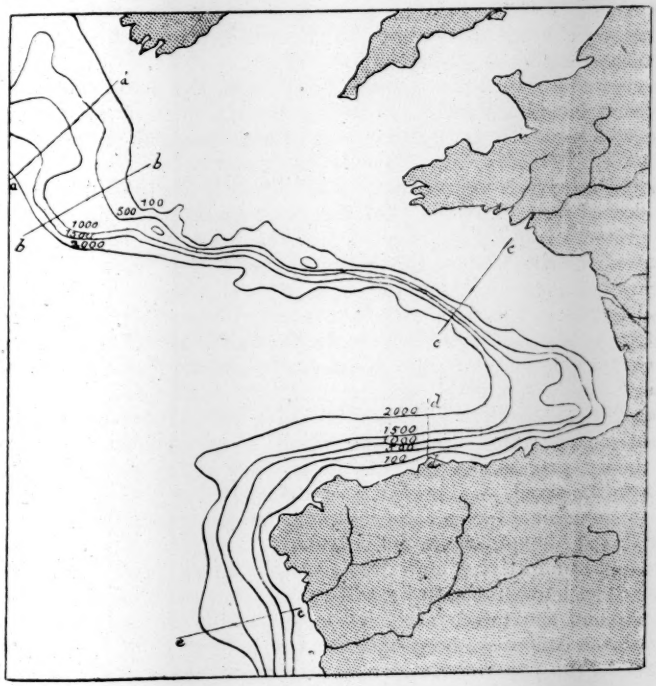


Fig. 3.—Submarine Contour-lines from Ireland to Portugal.

This apparent dependence of the breadth of the submerged shelf and the crowding together of the lower contour-lines on the existence of mud-bearing rivers, may be found repeated in many parts of the world where it is not obliterated by the redistributing power of the long-shore marine currents. The phenomenon is too universal to be assigned to submergence alone, though submergence in some places may modify or accentuate it, but must be the result of some ever acting but varying cause, such as the building up of new deposits from the materials denuded from the old.

The special effects upon the contour-lines immediately opposite the mouths of rivers can be demonstrated in many cases. Thus, no

the map (Fig. 3) already quoted, the river Minho is an example. This descends almost straight from the mountains of Asturias, and has a short course. It is therefore rapid and muddy, and the line *e–e* drawn from its mouth outwards shows the deflection *towards* the shore of the upper contour-line and *from* the shore of the lower contour-line, in almost too exact conformity with the theory. The Douro, though a longer river, has not so rapid a descent, and the inflection of the contour-lines opposite its mouth do not go beyond that of 100 fathoms, and thus do not appear on the map, but the charts show a marked inflection of the 50, 80, and 90 fathoms contour-line. Such smaller inflections must be similarly looked for in closely surveyed charts, and will be found, for instance, all round the coast of India.

There is less reason to quote examples of the actual occurrence of this expected inflection, as it has already been dwelt upon by those who regard it as the indication of a submerged river channel. We have seen, however, that for the production of a depression opposite the river's mouth there is no necessity for the river to have *once* flowed *in* it, provided that the river-current *now* flows *over* it and protects it from deposit. From the instances thus quoted we must except the Congo, the gorge opposite the mouth of which appears to be due to the same cause which guided the river to its actual course. This is shown by the gorge extending up the river itself, "a depth of 150 fathoms being found 20 miles within its mouth."[1] This "is not due to erosion by the river, for the current, strong though it is, does not extend more than 20 fathoms from the surface." The author here quoted, who like Godwin-Austen has himself accompanied the sounding-line, is so much in accord with the theory propounded in this paper that a further quotation is much to the point. "The existence and persistence of the cañon are due to an agency which prevents the mud brought down by the river being deposited along its axis. . . . . The bottom of the cañon, or gully, would thus resemble more nearly the original form of the bottom of the sea before the Congo discharged its waves into it than the flatter and shallower bottom on each side of it. In fact, the cañon has been built up, not hollowed out. In how far a crack in the crust of the earth may have had to do with the depth and position of this cañon it is impossible to speak certainly, but the preservation and accentuation of it are certainly due to the prevention of the deposit of sediment." This recognizes the agency here invoked, but does not apply it to the *production* of the gorge. It could not, in fact, have produced the whole of it, as the bottom of the shallower sea at the sides is marked in many places within forty miles of the land as 'stony.'

The bulging seawards of the lower contour-lines can only be looked for in large rivers or currents, or in specially rapid ones on quickly sloping shores: otherwise they are liable to be modified by oceanic currents. But a very remarkable bulge is seen in the case

[1] J. G. Buchanan: Scottish Geogr. Mag., vol. iii, p. 222.

of the La Plata, opposite the mouth of which the chart accompanying the Summary of Results of the *Challenger* Expedition shows a broad seaward deflection of the 2,000 fathom line. It is, of course, very difficult to assign every peculiarity of a contour to its proper cause, but in this case there is no remarkable submarine bulge along the whole eastern coast, except where theory would lead us to expect it. In the north coast there is also a longer bulge, corresponding to the Amazon, but driven westward by the equatorial current.

If the differences of depth near coastlines are due to the deposition or non-deposition of sediment, we might expect that where long-shore currents do not interfere the *lateral* boundary of a deposit should also be marked by a slope, and the interval between deposits from different sources by a broad channel. In this way we may *suggest* a reason for such peculiar features as the 'Fosse du Cap Breton,' 'the Baltic River,' and the 'Swatch of no ground.' The first of these may be the interval between the deposits from the Loire and Gironde and the slopes of the Pyrenees, where there is little deposit; the second, between the wide and ancient deposits which have covered the North Sea from the Elbe, Rhine, Thames, etc., and the almost unaltered boundary of South Norway; and the third, between the deposits reaching the Indian Ocean by way of the Hoogly and by way of the Brahmaputra respectively.

The views here propounded may now, I think, claim to be supported, not only by *a priori* arguments, but by many undeniable facts of nature of which they offer an explanation, and by several first-hand observers who have been led to adopt some part of them at least, as occasion served. But they are subversive of common assumptions in so far as these are applied to uniform deposits, and they alter entirely our means of interpretation of the sources of any stratum. The 'continental slope' has been called an escarpment, though the term was afterwards withdrawn; but according to these views it *is* an escarpment, but with this difference: it is an escarpment yet to be. When the marine deposits have been raised above sea-level and we stand facing the escarpment, we may be looking at the actual end of the deposit, except for later weathering backwards, and we must look along the present dip slope towards the land whence the material of the strata has been derived.

When limestones give place along the strike to clays we must not look for the shore in that direction, but in a direction perpendicular thereto, nor must the sea be supposed to have been deeper beneath the limestone, but possibly shallower, the unevenness of the sea-bottom having caused the clays to be deposited elsewhere.

Neither must we regard the existence of a well-marked continental slope as a proof of depression, it may be of 6,000 feet or less, of a land of great mobility; but look on it as a proof of stability, which allowed the slowly deposited sediments to retain the same limit for so long a time. The existence of the continental slope in this way speaks against the hypothesis of a glacial submergence.

On the other hand, we must be careful not to push theoretical results beyond their proper limits. Deposits may be altered in

shape by various causes before we can examine them: currents may deflect the deposits from where we should expect them: and not all the contours of the sea-bottom are caused by deposits alone. All that can be said is that the tendency of uniform deposits to thicken before ceasing must always be present, and must modify any result which may be due in part to other causes: while under some circumstances this cause may be expected to be the only one which gives the deposit its form.

VII.—On two Sections of the Rhætic Rocks in Worcestershire.

By L. Richardson, F.G.S.

A DESCRIPTION of two Rhætic sections forms the subject of this communication. In a county where but one section has been described this is a considerable addition to our knowledge of the series. It is remarkable that the section at Crowle has not been previously noticed; it may be easily examined, and not like the only recorded section in the railway-cutting at Dunhampstead, which is necessarily difficult to obtain access to. The Crowle section is situated at the junction of the road from Oddingley with that from the house marked as "Frisland" on the Geological Survey map. The beds are affected by a fault having a downthrow of about 14 feet.

Section at Crowle, near Worcester.

			ft.	ins.
Lower Rhætic, or Zone of *Avicula contorta*.	7.	Shales, black.		
	8.	Sandstone, yellowish-white.		
	9.	Shales, brown, imperfectly laminated; uppermost 4 ins. black	1	2
	10.	Sandstone, several layers separated by shaly partings; *Gyrolepis Alberti*, teeth of some actinopterygian fish (possibly *Gyrolepis*), annelid-tracks, ripple-marks, and a vertebra	0	9
	11.	Shales, black; numerous sandstone layers, uppermost 3 ins. brown	1	8
	12.	Sandstone (Bone-bed-equivalent), yellowish-white, micaceous, fissile; *Schizodus* (casts), ripple-marks, and annelid-tracks	1	1
	13.	Shales, black (weather grey), laminated, arenaceous towards base	2	11
	14.	*a*. Sandstone, yellowish-white, micaceous, fissile; casts of *Schizodus* somewhat abundant — especially in the lowest layer	0	6–8
		b. Sandstone layers with shaly partings	0	2–3
		c. Yellow clayey deposit	0	1
Upper Keuper.	I.	'Tea-green Marls.' Creamy white and slightly green marls with three harder bands of marlstone: estimated at ...	31	0
	II.	Red marls with bluish zones and blotches.		

The 'Tea-green Marls' are well exposed, and comprise creamy white marls, slightly green in places, with harder bands, the marlstone composing these harder bands having a most irregular fracture. At the base of the Rhætic is a yellow clayey deposit in which no organic remains were observed. The junction of the Rhætic and Keuper series is sharply defined, as is also the case in North-West Gloucestershire. About 3 inches of thin sandstone layers with intercalations of shaly matter separate this yellow

clayey deposit from a more massive bed of sandstone, in the bottom layer of which casts of *Schizodus* are somewhat abundant. In the black shales which succeed no fossils were observed, but they, in common with the rest of the beds in this section, have suffered much from atmospheric influences. These shales are capped by a massive bed of sandstone, the equivalent of the true Rhætic Bone-bed. In North-West Gloucestershire this bed undergoes numerous lithic changes; for example, in the cliff section at Wainlode, near Gloucester, it passes from a thin hard pyritic bed, replete with fish scales and teeth, into a non-ossiferous sandstone about a foot thick. In the latter form it is visible in the shallow cutting on the Tewkesbury and Ledbury road at Bushley, and again at Bourne Bank, near Defford, where it has increased in thickness to 2 feet. Another exposure of the same bed is obtainable in the lane cutting $\frac{3}{4}$ mile north of Croome D'Abitot Church, and is seen to be separated by 2 feet 10 inches of black shales from the 'Tea-green Marls.' No more good exposures are afforded until we reach Crowle; but in the intervening area another deposit of sandstone has made its appearance, separating the black shales from the Upper Keuper marls. The maximum thickness of this arenaceous deposit is 1 foot, and in the railway cutting at Dunhampstead, near Droitwich, permission to examine which was kindly given me by the Midland Railway Company, it is about the same. On the eastern borders of the county at Marl Cliff, however, no such deposit is present, and the Bone-bed-equivalent, under an inch in thickness, rests upon 2 feet of black shales, and this latter deposit upon about 24½ feet of 'Tea-green Marls.' With a development of the Bone-bed, such as that at Marl Cliff, we might, from experience, have expected to see it highly ossiferous, but, excepting a very few fish scales, such is not the case. Indeed, the only locality in Worcestershire where a really ossiferous Bone-bed has been observed is at Hob Lench, and Mr. R. F. Tomes, F.G.S., informed me that Mr. Kershaw and he obtained from a well here the specimens now in the Museum of the Victoria Institute at Worcester, presented to that Museum by Mr. Tandy.

To return to the Crowle section, 1 foot 8 inches above the Bone-bed-equivalent is a series of sandstone layers separated by shaly partings, collectively 9 inches in thickness, and constituting the most fossiliferous deposit in the section. It was in what I consider to be the equivalent deposit at Deerhurst that I found a new species of *Heterastræa*; the only authentic record, I am told by Mr. Tomes, of a coral from the zone of *Avicula contorta*. The other section to be noticed is in the Duke of Orleans' grounds at Woodnorton, near Evesham. At the hill locally known as the 'Blue Hill' a bridle-path passes through a cutting under a public road. Mr. Wasley, Head Keeper to the Duke of Orleans, kindly conducted me to the section. The bank had been smoothed down and the section was rapidly becoming obscured, so that it was impossible to examine the shaly deposits for fossils. The limestone bands, however, were very prominent.

Section at Woodnorton, near Evesham.

		ft.	ins.
Lower Lias (*pre-planorbis*).	Limestone, bluish-grey; *Ostrea liassica*	0	3
	Clay, yellowish, marly	2	7
	Limestone, in two layers, bluish-grey, shelly; *Ostrea liassica*, *Modiola minima*	0	4
Upper Rhætic.	1. Shales, brown, thinly laminated	2	0
	2. Limestone, hard, bluish-black; *Modiola minima*	0	5
	3. Shales, grey and greenish-grey, laminated in places, usually non-laminated, marly ...	5	4
	4. Limestone (*Estheria*-bed), creamy yellow, argillaceous; *Estheria minuta* var. *Brodieana*, *Naiadita lanceolata*	0	3–5
	5a. Shales, grey, greenish-grey, and yellowish, thinly laminated in places, usually non-laminated, marly; shell fragments	14	6
Lower Rhætic, or Zone of *Avicula contorta*.	*5b*. Shales, dark-coloured		
	6. Limestone nodules, containing *Schizodus Ewaldi*; resting upon a laminated limestone full of shell fragments; *Pecten valoniensis*, *Schizodus Ewaldi*, *Protocardium*, and *Pleurophorus* ...	0	8
	7. Shales, black, laminated ... ... (visible)	0	6

This section is the only one of the Upper Rhætic stage in the county, and accordingly is of considerable importance. At Wainlode Cliff the Upper Rhætic is 13 feet 4 inches in thickness, here it is about 19 feet 7 inches, i.e., deducting 3 feet from the thickness of the lowest deposit of the upper stage, which would probably be found to belong to the *Avicula contorta* zone or Lower Rhætic had it been possible to investigate it for fossils.

Concerning the nature of the deposit above the beds exposed at Crowle and below those seen at Woodnorton the section at Dunhampstead affords some assistance. These intervening beds at Dunhampstead are as follows:—

		ft.	ins.
Lower Rhætic.	6. Limestone nodules	0	10[1]
	7. Shales, black, clayey, pyritic (4 feet less 1 ft. 5 in.)	2	7[1]
	8. Sandstone, brown, micaceous; *Modiola minima*, *Schizodus* ...	0	3
	9. Shales, brown, clayey	1	2
	10. Sandstone, yellowish (weathering brown), micaceous; *Modiola minima*, *Schizodus*, annelid-tracks, fish scales	0	5

If this record be read with those made at Crowle and Woodnorton, a generalised section will be had of the Rhætic series as it occurs in Worcestershire.

VIII.—Scratches on Minerals in Thin Sections.

By H. Stanley Jevons, M.A., F.G.S., Lecturer in Mineralogy and Demonstrator in Geology in the University of Sydney.

THERE is an exceedingly simple method of distinguishing biotite from brown hornblende in thin sections under the microscope, which, so far as I am aware, has not yet been described. As it may possibly be of assistance to beginners, I submit a brief account thereof.

Let any section of biotite which shows the cleavage and pleochroism be turned between crossed nicols until the light is almost, but

[1] On authority of Mr. W. J. Harrison, F.G.S.: *vide* Proc. Dudley and Midland Geol. Soc., vol. iii, No. 5 (1877), p. 121.

not quite extinguished. When closely examined with a low power it will be seen to be irregularly speckled with small spots and patches of light, generally oval or elongated in shape, which often give its surface a rough or shagreened appearance. These spots are still visible if the section be turned to complete extinction, and are then cut in two longitudinally by a dark line. If the section be further turned to the other side of the position of extinction they resume their original shape, but are seen to have changed their position slightly.

The cause of this want of uniformity in extinction is easily ascertained by examination with a high power. Wherever these light spots occur the cleavage lines are seen to be bent laterally from their normal course into a V-shape, doubtless by the scratching action of the coarser grains of the polishing material during the final stage in the grinding of the section. The V's piled upon one another simulate a cross section of a pointed anticline or syncline of great vertical range.

It is evident, therefore, that when the section as a whole is at its position of extinction, the mica along both arms of the V lies in such directions that light will get through it. The dark line before mentioned as cutting the spots longitudinally when the section is in this position passes through the apices of the V's, and is due to the fact that between their two arms the mica must lie for a short distance in its original direction, now the direction of extinction.

The spots are brightest, however, when the section as a whole is not quite extinguished, for then one arm of the V is completely extinguished, whilst the other is well on towards its position of maximum illumination. On turning the biotite from one side to a similar position on the other side of its direction of extinction the effect is similar, but reversed; what was previously the darker arm of the V has become the lighter, and *vice versâ*. The same effect is visible in less marked degree with only one nicol in use, on account of the strong pleochroism of biotite. The angular displacement of the arms of the V is sufficient distinctly to alter the amount of absorption.

The appearance described is presented, in my experience, only by such minerals as are soft, and have a very good cleavage. It is visible in biotite, muscovite, and talc, and a few other soft minerals rare in rock sections. Practically, every section of these minerals which I have examined shows the effect, excepting those parallel to the cleavage, and I have *never* seen it in hornblende or any member of the pyroxene or amphibole family.

If the attention of the beginner has once been called to these scratches, he never mistakes hornblende for biotite; and only under the rarest circumstances connected with the preparation of the section could he mistake biotite for hornblende, even if relying solely on this test. To the more advanced student the appearance may occasionally prove of service in the identification of minerals, as evidence of their softness.

IX.—Note on an Unmapped Toadstone Bed in the Derbyshire Mountain Limestone.[1]

By Samuel Moore, Esq.

In the Summer of 1901 I found in a pasture, between Oxlow Rake and Cop Round (IX S.E.), some blocks of Toadstone in a bed of clay that has all the characteristics of decomposed Toadstone. The clay was being dug for puddling a new mere, and the deposit is well known to natives. I traced the outcrop south-west to Starvehouse Mine, and my inference that the clay was decomposed Toadstone was soon verified by the Toadstone itself coming to daylight and replacing the clay. I did nothing more that year, but in the Summer of last year I continued to follow the bed, and have traced it as far as Bushy Heath House (XV N.E.), a distance of about two miles from its starting-point.

The outcrop starts as a clay bed, at the southern end of an enclosure called Old Moor (IX S.E.), at a point about 50 yards south-east of two old mine shafts, and where a spring issues from under the limestone scarp, at a height of 1,400 feet above mean sea-level (by aneroid). Thence it runs in a general south-west direction for half a mile, along a line of five springs at the base of the limestone escarpment, to the north wall of Starvehouse Mine (IX S.E.), which it cuts at an altitude of 1,500 feet.

There is no throw at the Starvehouse lode, and the bed contours round the point of Cop Round and crosses Dick Lane 30 yards from the summit gate (1,510 feet); from there it runs south-east with the dip of the limestone to Moss Rake (IX S.E.), the base passing just above the letter *n* of Piece Barn on the map. It reaches the lode at 1,350 feet. Here the bed is faulted and thrown up to the north, and the top starts afresh about 180 yards up the lode, at an altitude of 1,420 feet.

The top then runs, along the base of a limestone scarp, to a point beneath the Old Roman Camp on Batham Gate (IX S.E.), crosses the road at the gate to The Holmes (XV N.E.), and runs down, through the farmyard, to the continuation of the Shuttle Rake lode, below the old Calve Stones Mine plantation.

There appears to be no fault at this lode, but the bottom of the valley is flat and filled with superficial deposits which conceal the outcrop.

From The Holmes the bed is traceable still in a general south-east direction, as far as an old quarry (XV N.E.) at an altitude of 1,300 feet, and 300 yards N.N.W. from Bushy Heath Farmhouse. From there to Chap Maiden Mine (1,276↗) the traces are indistinct, owing to flat ground. Beyond this point I have not at present traced it, but next Summer I hope to do so.

The bed of Toadstone here described lies above the topmost bed shown on the 1 inch map of the Geological Survey, and is separated from it by nearly 150 feet of solid limestone. It varies in thickness:

[1] See Geol. Surv. Map 81 N.E., original 1 inch scale; Maps IX S.E. and XV N.E., 6 inch Ordnance Survey.

at the start on the Old Moor I should put it at 15 or 20 feet of clay, but it thickens in a south-west direction, and at Cop Round I estimate it to be 60 to 80 feet thick, which thickness it maintains up to the Moss Rake. After that it seems to thin out; but just above The Holmes and beneath the Roman Camp it crops out strongly again and is about 80 feet thick, if not more. From The Holmes to Bushy Heath I could not ascertain the thickness. At Cop Round some blocks are concretionary, and I have not noticed any columnar structure. The bed seems to decompose into clay very freely where it is not thick.

I wish to draw attention to this bed of Toadstone because I think it will be of importance in the stratigraphy of the Toadstone beds near Tideswell, Litton, and Wheston; and further, in order that the bed may not be overlooked in any future resurvey of the country: it is not a prominent bed like the one immediately below it.

With regard to this latter bed I may as well point out what strikes me as an error on the part of the Geological Survey. On their 1″ map 81 N.E. the bed is made to start at a point between Eldon Hole (IX S.E.) and the summit of Eldon Hill, at 1,480 feet altitude, where the limestone beds dip true E. 40° S., at an angle of 6°; it is then drawn to run down into Cunningsdale, round Oxlow, across Oxlow Rake to Oxlow Dam, and on to The Cop Farmhouse. In the length between Eldon Hill and Oxlow Dam I cannot find any evidence of an outcrop of Toadstone, or even of clay that might be decomposed Toadstone. As one traces the bed westward from The Cop Farmhouse (IX S.E.) it appears to thin out, and vanish at a spring just outside the south-west corner of Starvehouse Mine. Moreover, the line as drawn in its descent from Watts Plantation (IX S.E.) to the bottom of Cunningsdale, and in its ascent again to Oxlow Rake, does not follow the dip, but cuts across the limestone beds in a manner not usual with the Toadstone in this part of the country.

REVIEWS.

I.—The Palæontographical Society of London.

The Annual Volume of the Palæontographical Society for 1902: Vol. LVI. 4to. (London: printed for the Society. Dulau & Co., Agents for the Society, 37, Soho Square, W. Price to Subscribers 21*s.* per annum.)

AGAIN it is our pleasant task to welcome the issue of another volume (larger than usual) of this Society's admirable publications, which has for its object the description and figuring of all the known British fossils.

When its heroic founders set out on this enterprising task, nearly sixty years ago, they were doubtless as courageous as Jason and his Argonauts when they set sail in the good ship *Argo* to fetch the golden fleece; but, like Jason and his Grecian heroes, they must soon have found that they had undertaken a very arduous

task; many of the brave workers have died since then, yet the task still remains unfinished. Yet notwithstanding our losses, the heroes of our modern *Argo* are an indomitable body, and its ranks are ever being renewed by fresh palæontological volunteers.

In our last number we recorded the death of our late Secretary, Professor Wiltshire, who for 36 years had carried on the administration of the Society. His place is now filled by Dr. Arthur Smith Woodward, F.R.S., while many new contributors (himself among the number) have been added to our body.

In the present volume Professor S. H. Reynolds takes up the unfinished work on "The Pleistocene Mammalia," commenced by Messrs. Boyd Dawkins & Sanford in 1864, and continued till 1871; since then, with the exception of a short account of the Pleistocene Cervidæ by Professor Boyd Dawkins in 1886, the original authors have abandoned their work, which is now taken up by Mr. Reynolds. The present fasciculus forms part 1 of vol. ii, and is devoted to "The Cave Hyæna," the illustrations being mostly taken from the wonderful series of Hyæna remains from Wookey Hole in the Mendips, now preserved in the Taunton Museum. Six out of the fourteen plates were executed long ago by the late Mr. W. Bidgood, formerly curator of the Taunton Museum; the others are newly drawn by Mr. J. Green, one or two from the British Museum collection and others from that of Owens College, Manchester. The Society is to be congratulated upon securing the services of Professor Reynolds for this important work on vertebrate palæontology.

The next monograph is a new one on the "Fossil Fishes of the English Chalk," by Dr. Arthur Smith Woodward, and is illustrated by thirteen very fine plates by Mr. A. H. Searle and twelve drawings and restorations of *Eurypholis*, *Odontostomus*, *Chlorophthalmus* (recent), *Sardinioides*, *Hoplopteryx* (*Beryx*), *Beryx splendens* (recent), *Berycopsis elegans*, *Aipichthys*, admirably drawn by Miss G. M. Woodward. One plate is an autotype of a group of Chalk fishes of the genus *Hoplopteryx* from the Beckles Collection in the British Museum, by Mr. Green. Fifty-six pages of descriptive letterpress are issued with this very important memoir.

The third monograph is a continuation of the "Cretaceous Lamellibranchia of England," by Henry Woods, M.A., part iv, being devoted to the Chalk Pectens. The twelve plates are very ably rendered by Mr. Hollick.

The concluding monograph is a continuation of that on British Graptolites by Gertrude L. Elles & Ethel M. R. Wood, edited by Prof. Charles Lapworth, F.R.S. There is an interesting introduction, and the following genera are described and figured: *Tetragraptus*, *Schizograptus*, *Trochograptus*, *Holograptus*, *Dichograptus*, *Loganograptus*, *Clonograptus*, *Temnograptus*, *Bryograptus*, *Azygograptus*, and *Phyllograptus*. We confess to a feeling of disappointment in the production of the very careful original drawings by process in these plates. We prefer an accurate, clear, distinct *outline* to each figure. We are quite sure these are *most accurate*, but they are neither so

clear nor so distinct as we could desire; and there is a dread that this highly-surfaced paper will, after some years, perish, being overloaded with kaolin. There are quite a number of process-blocks in the text, some of which give *most clear details*, such as we desire, but others are less satisfactory. We look back with admiration to Professor Lapworth's own original drawings in this MAGAZINE, the Quart. Journ. Geol. Soc., and elsewhere, which are to our minds more satisfying, notwithstanding the great labour which the authors have expended on the present monograph.

Taken as an annual issue, this is a fine volume, containing, as it does, 207 pages of descriptive text, 47 plates, and numerous illustrations in the text, together with 30 pages of introductory matter, the whole of which can be obtained for the small subscription of one guinea.

We expect to find that, after this announcement, Messrs. Dulau & Co., 37, Soho Square, and the Secretary, Dr. Arthur Smith Woodward, will have a busy time in satisfying the demands of the many new and enthusiastic subscribers who will send in their names and cheques!

II.—SHERBORN'S INDEX NOMINUM ANIMALIUM, 1758–1800.

INDEX ANIMALIUM, sive index nominum quæ ab A.D. MDCCLVIII generibus et speciebus animalium imposita sunt societatibus eruditorum adjuvantibus, a CAROLO DAVIES SHERBORN, confectus, sectio prima a kalendis januariis MDCCLVIII usque ad finem Decembris MDCCC. Cantabrigiæ e typographio academico MDCCCCII. Roy. 8vo; pp. lix, 1196. (London: C. J. Clay & Sons, Cambridge University Press Warehouse, Ave Maria Lane, 1902. Price 25*s.* net.)

THE Syndics of the Cambridge University Press having undertaken the publication of the first part of the "Index Animalium," to the preparation of which Mr. C. Davies Sherborn has devoted so many years, it seems desirable to explain the nature of the work upon which so much time, labour, and expense have been bestowed. The object of the Index is to provide zoologists with a complete list of all generic and specific names given by authors to animals, both recent and fossil, since January 1st, 1758, the date of the tenth edition of Linnæus' "Systema Naturæ." With each name is given an exact date, and a reference intelligible to the layman as well as to the specialist.

The British Association appointed a Special Committee to watch over the inception and progress of the work, the preparation of which was undertaken in 1890.[1] Financial support has been given by the British Association, the Royal and the Zoological Societies, while the authorities of the British Museum (Natural History) have afforded continual assistance.

[1] An interval of three years was unfortunately lost by the author from ill-health, so that the completion of this first volume has occupied a period of eight years, a no inconsiderable task for one man to undertake and carry through to completion.

Similar works have been produced before, but no one book has yet appeared attempting to supply references to all names given to both fossil and recent animals, nor has any definite attempt been made heretofore to fix an accurate date to each name.

This work will, of course, supersede Agassiz and Scudder, and must be absolutely indispensable to zoologists of all countries. It will be to the student of animal life what the "Index Kewensis" is to the botanist, and, indeed, far more, as the last-named work refers only to Phanerogams, whereas the "Index Animalium" includes all groups of animals, and both recent and fossil forms. The portion of the work already completed and now issued from the press of the University of Cambridge covers the period from 1758 to 1800, consists of 61,600 entries, and fills 1,196 pages royal octavo.

In 1897 Dr. Sclater suggested to the Committee that, in view of the long time that must elapse before the completion of the whole manuscript, which it was proposed should embrace the period from 1758 to 1900, it would be well if a portion were prepared and published as a specimen of the whole. This suggestion has been acted upon, and there is now offered to the consideration of zoologists that part covering the years 1758 to 1800 inclusive. During the progress of this earlier portion of the manuscript it became apparent that a great deal of the literature required was not to be found in England, and it is satisfactory to be able to say that, with a few unimportant exceptions, the bulk of the 1,300 volumes required have either been seen, or else seen and acquired for some one or other of the libraries accessible to the public. "The search for these volumes," says Mr. Sherborn, "has been not the least interesting part of my labours. Such as have eluded my search are mentioned in 'Libri desiderati,' where a complete enumeration of the books consulted has been given in a second list, this seeming to be advisable and useful; and at the same time such notes are given on them as would be of use to those who might wish to consider or purchase them."

"I would like to say that, with the exception of some fifty entries kindly made for me by my friend Dr. Bather in Stockholm, and by Professor Perez in Bordeaux, every entry has been recorded from the original, arranged, sorted, checked, and passed for press by myself. I therefore beg the indulgence of those who use this book for any error of commission or omission that may be found. And I may add that a note of any such error will be especially valuable for inclusion in the second part of my 'Index Animalium.'"

We heartily commend this important work to the attention of all librarians, curators, and zoologists in all the world, for none can work without it. It is like the "Postal Guide," "Bradshaw," or "Whitaker's Almanac," a handy book of reference, containing the scientific names of beasts of all kinds, and must be on the bookshelf of every library embracing Natural History.

We earnestly hope the author's valuable life may be spared to see the completion of the next century of his Index through the press, the manuscript for which is already far advanced.

The later years will be not only of far greater importance to zoologists, but will be a thousandfold more prolific in the number of works to be referred to, and of the objects described in them, than those of the previous half-century. We compliment the author on the grand volume he has produced, and the Syndics of the Cambridge University Press for having printed it in so accurate and admirable a manner.

REPORTS AND PROCEEDINGS.

Geological Society of London.

I.—December 3rd, 1902.—Professor Charles Lapworth, LL.D., F.R.S., President, in the Chair. The following communications were read :—

1. "On some Well-sections in Suffolk." By William Whitaker, Esq., B.A., F.R.S., F.G.S.

Notes of thirty-one new wells have accumulated since 1895, some of them giving results which could not have been expected. A trial boring for the Woodbridge Waterworks Company gave a depth of 133¾ feet down to Eocene beds, and a thickness of Crag about double of any before observed in the neighbourhood. An analysis of the saline, hard water yielded is given. Three explanations are suggested: a channel, a huge 'pipe' in the Chalk, or a disturbance such as a fault or a landslip; but the author is not satisfied with any of them. Two borings at Lowestoft show that Crag extends to a depth of 240 feet in one case and over 200 feet in another, confirming estimates of Mr. Harmer and Mr. Clement Reid. In one of these, Chalk was reached at 475 feet. Three other wells in the neighbourhood confirm the great depth of the newer Tertiary strata. Sections are also given from the following places :—Boulge, Hitcham Street, Ipswich (corroborating the evidence for a deep channel filled with Drift given by the section at St. Peter's Quay, New Mill), Shotley, Stansfield, and Brettenham Park. The last shows the greatest thickness of Drift recorded in the county, namely, 312 feet.

2. "The Cellular Magnesian Limestone of Durham." By George Abbott, Esq., M.R.C.S., F.G.S.

The Permian Limestone covers about 1¼ square miles near Sunderland; it alternates with beds of marl containing concretionary limestone-balls, and attains a thickness of 65 feet or so. The cellular limestones frequently contain more than 97 per cent. of calcium-carbonate. Magnesium-carbonate occupies the interspaces or 'cells' of this limestone, and also the spaces between the balls. The hundred or more patterns met with in it can be arranged into two chief classes, conveniently termed honeycomb and coralloid, each with two varieties; and each class has four distinct stages, both classes having begun with either parallel or divergent systems of rods. The second stage is the development of nodes at regular distances on neighbouring rods; and these in the third stage, by

lateral growth, become bands. Finally, in the fourth stage the interspaces become filled up. The upper beds are usually the most nearly solid. In the coralloid class the nodes and bands are smaller and more numerous than in the honeycomb class. In both classes tubes are frequently formed. The rods have generally grown downwards, but upward and lateral growth is common. A section of Fulwell Quarry is given.

II.—December 17th, 1902.—Professor Charles Lapworth, LL.D., F.R.S., President, in the Chair. The following communications were read:—

1. "Note on the Magnetite Mines near Cogne (Graian Alps)." By Professor T. G. Bonney, D.Sc., LL.D., F.R.S., F.G.S.

These mines have been worked probably since Roman times, but are now almost deserted. They are situated in the Val de Cogne, one of the larger tributaries of the Val d'Aosta from the Graian Alps. The author, in company with the Rev. E. Hill, last Summer examined two localities where the ore has been worked. At one, the Filon Licone, the mass of magnetite is probably about 80 or 90 feet thick and some five times as long. At the other place, the Filon Larsine, the mass apparently is not nearly so thick. The ore is a pure magnetite, jointed like a serpentine, a thin steatitic film being often present on the faces. At both localities the magnetite is found to pass rapidly into an ordinary serpentine, the transitional rock being a serpentinized variety of the cumberlandite described by Professor Wadsworth in his "Lithological Studies." The serpentine is intercalated, like a sill, between two thick masses of calc-mica-schists, with which green schists (actinolitic) are as usual associated, no doubt intrusively. All these represent types common in the Alps. The author discusses the relations of the magnetite and serpentine, which, in his opinion, cannot be explained either by mineral change or by differentiation *in sitû*, but indicate that a magnetitic must have been separated from a peridotic magma at some considerable depth below the surface, and the former, when nearly or quite solid, must have been brought up, fragment-like, by the latter; as in the case of metallic iron and basalt at Ovifak (Greenland).

2. "The Elk (*Alces machlis*, Gray) in the Thames Valley." By Edwin Tulley Newton, Esq., F.R.S., F.G.S.

During the construction of the Staines Reservoirs some mammalian remains were obtained from the alluvium of the Wraysbury River, near the Thames at Youveney. At the request of Mr. T. I. Pocock, of the Geological Survey, who is working in the district, the engineers, Messrs. Walter Hunter and R. E. Middleton, courteously submitted their specimens to the author, who recognized among them the skull and antlers, with other parts of the skeleton, of a true elk (*Alces machlis*). These are described; allusion is made to the earlier records of this animal in Britain; and its distribution in time in this country, on the continent of Europe, and in North

America is also discussed. It appears that *Alces machlis* has been frequently found in peaty deposits in many parts of Great Britain and on the continent of Europe, but never in Britain in association with the mammoth; and it seems probable that in Europe and North America it was a rare animal in Pleistocene times, if indeed it was present before the close of that period.

3. "Observations on the Tiree Marble, with Notes on others from Iona." By Ananda K. Coomáraswámy, Esq., B.Sc., F.L.S., F.G.S.

The gneiss near Balephetrish has a general south-westerly and north-easterly trend, and the limestone occurs in it as lenticles of various sizes, having a similar foliation. Descriptions of pink, grey, and white varieties of the limestone in this locality are given. The inclusions comprise those of gneiss containing quartz, felspars, hornblende, augite, scapolite, and sphene as characteristic minerals, and mineral aggregates consisting of sahlite, coccolite, scapolite, sphene, apatite, calcite, and mica. The contact-phenomena are not specially well displayed, but several instances are described; and in these the minerals of the modified gneiss interlock with those of the modified limestone, and there is no actual line of junction seen under the microscope, although an abrupt change is evident. The dynamic phenomena include the rounding of the minerals (frequently, however, an original character) and the formation of 'augen.' The carbonates are present as a fine-grained granular matrix, the result of the breaking down of larger grains, probably at a temperature not above 300° C., as indicated by the experiments of Adams and Nicolson. Although there are exceptions, gneiss-inclusions and mineral aggregates have usually been protected from the effects of extreme pressure. The description of minerals includes carbonates, pyroxene, amphibole, forsterite, scapolite, sphene, mica, apatite, and spinel. White, greenish, and black marbles are described from Iona, where they are associated with actinolite-felspar-schists and others; they are included in the gneiss. Sedimentary rocks suggestive of Torridon Sandstone occur along the eastern shore of Iona.

III.—January 7th, 1903.—Professor Charles Lapworth, LL.D., F.R.S., President, in the Chair. The following communication was read:—

"On the Discovery of an Ossiferous Cavern of Pliocene Age at Dove Holes, Buxton (Derbyshire)." By William Boyd Dawkins, M.A., D.Sc., F.R.S., F.G.S., Professor of Geology in Owens College, Victoria University (Manchester).

The Carboniferous Limestone, riddled with fissures and potholes, in the neighbourhood of Dove Holes, has from time to time, in the course of the working of the quarries, yielded remains of extinct mammalia of Pleistocene age. The latest discovery of a group of mammalia, of far higher antiquity than the Pleistocene, is now brought before this Society. The Victory Quarry, Bibbington, in which the discovery was made, is excavated in a rolling plateau of

Carboniferous Limestone, from 1100 to 1200 feet above Ordnance-datum, and forming at this spot the water-parting between the tributaries of the Goyte, flowing past Chapel-en-le-Frith westward into the Mersey, and those flowing southward and eastward, past Buxton, to join the Derwent. It is a little to the north of the centre of the divide. On the western side the limestone dips at an angle of 15° underneath the Yoredale sandstones and grit, which form the lower half of a range of hills, extending southward to Buxton and beyond. The upper half is composed of shales and sandstones of the Millstone Grit Series, that rise in Black Edge to a height of 1662 feet. The drainage of the eastern slope of these hills passes downward, until it arrives at the limestone, where it sinks into the rock, through the many swallow-holes which mark the upper boundary of the limestone. There are no surface streams in the limestone in the immediate neighbourhood of the Victory Quarry, which, from its position on the divide, could not, under existing geographical conditions, receive the drainage from this western range of hills or any other source.

In the course of working the quarry, in the beginning of 1901, a cave was discovered, and fully exposed in the course of 1902. It was about 90 feet long, 15 feet high, and 4 feet broad. It ran nearly horizontally north and south, and consisted of a large chamber and a small passage, both eroded in a master-joint. On the south it contracted to a dead end, now quarried away. Its continuation to the north is obscured by a great accumulation of broken rock and clay, which has not yet been removed. It was filled with a horizontally stratified red clay, containing angular and rolled pebbles of limestone, and a few sandstone pebbles from the Millstone Grit and Yoredale rocks. There were also a few pebbles of white vein-quartz and of quartzite. Scattered through the mass were mammalian bones and teeth, some water-worn and others with sharp fractures. The contents had been clearly introduced into the cave by water, flowing under geographical conditions which no longer exist.

The mammalian remains belong to the following species :—

Machairodus crenatidens, Fabr.
Hyæna, sp.
Mastodon arvernensis, Croiz. & Job.
Elephas meridionalis, Nesli.
Rhinoceros etruscus, Falc.
Equus stenonis, Nesli.
Cervus etueriarum, Croiz. & Job.

All these species are found in the Upper Pliocene deposits of France and Italy, and undoubtedly belong to that age. The *Mastodon*, elephant, rhinoceros, and horse occur also in Britain in the Upper Pliocene deposits of the Crag.

Some of the bones present the characteristic teeth-marks of the hyænas; and the preponderance of the remains of the young over the adult mastodons points to the selection by the hyænas, who could easily master the calves, while they did not as a rule attack the large and formidable adults. The author has observed a similar selection in the case of mammoths in hyæna-dens, into which the remains had been brought by those cave-haunting animals. He

therefore concludes that the animal remains have been washed out of a hyæna-den, which then existed at a higher level, and carried down deep into the rock, into the cave in which they were found, along with the clay and pebbles brought down in flood-time from the Yoredale and Millstone Grit hills.

The area of the Victory Quarry must then have been at the bottom of a valley, instead of in its present position on the divide. The denudation of the limestone which has taken place since that time is estimated at not less than 330 feet—an amount sufficient to destroy the ravine formed by the stream above the bone-cave, and all the caves and rock-shelters in the district, which were accessible to the Upper Pliocene mammalia.

The author appends a map illustrating the physical geography of the British Isles in Upper Pliocene time. In it the British area is represented as joined to the Continent by a barrier of land, extending from the Straits of Dover, westward, as far as the 100 fathom line in the Atlantic, which sweeps southward from Scandinavia, off the West of Ireland, into the Bay of Biscay. There were then no physical barriers to forbid the migration of *Machairodus*, *Mastodon*, *Elephas meridionalis*, and the rest, from Central and Southern France into Britain. They could find their way freely from the valleys of the Loire and the Garonne, across the valley now occupied by the English Channel, into England and, it may be added, Ireland. Over this area the animals migrated in the Upper Pliocene age. The discovery of a few of them in Derbyshire is to be looked upon as a monument of their former existence over the whole of this region. It is also a striking example of the great destruction of the surface which has taken place since that time, and of the imperfection of the geological record. It is the only cave in Europe that has yielded remains of the remote Pliocene Epoch.

CORRESPONDENCE.

PTERASPIS IN NORTH CORNWALL.

Sir,—In Dr. H. Woodward's description of *Homalonotus Barratti*, n.sp., in your last number he mentions that I recorded the occurrence of *Pteraspis* at Trevone in a paper which appeared in your Magazine, Dec. IV, Vol. VII, p. 146. This error happened owing to a stupid oversight of my own. I did find a portion of a Ganoid fish in the blue shales of Trevone which was recognized by Dr. Smith Woodward as undoubtedly Devonian, and belonging to a genus not yet described. Specimens of the same form, he said, were in the late Mr. Pengelly's collection. The plate showed the internal structure and the surface was ornamented with small bosses, but he said it was distinct from *Steganodictyum* (*Pteraspis*), Ray Lankester.

In my table showing the Distribution of Fossils on the North Coast of Cornwall, south of the Camel, published in the Transactions of the Royal Geological Society of Cornwall, 1901, vol. xii, part 7, *Pteraspis* is not recorded north of Bedruthan.

Since this table was published I have obtained from Bedruthan a slab on which there are fragments of *Pteroconus mirus*, and another organism of which Dr. Smith Woodward writes: "I think the reticulated piece belongs to *Pteraspis*, but it would have been more satisfactory to find the outer striated layer." HOWARD FOX.

FALMOUTH, *January 5th*, 1903.

THE GEOLOGY OF BARBADOS.

SIR,—My extended acquaintance with the geology of Barbados has led me to concur fully in the last paragraph of the recent article on the subject by Professor Harrison and Mr. Jukes-Browne, and especially in the admission of these authors that "fresh observations are required." For some weeks prior to the appearance of the article, indeed, I had been in correspondence with one of the authors of the article, who proposed a joint re-examination of the ground; I immediately accepted the suggestion, and began planning another trip to Barbados with the object of demonstrating my observations on the ground, and, if practicable, making additional collections of fossils; and I had hoped that public discussion would be withheld pending this appeal to the court of field observation. In view of the prospective meeting on the ground, I am content to withhold detailed criticism of the article in question, and especially of the restricted view taken by the authors, who seem satisfied to discuss the geological history of a single spot in a great province without reference to the records presented by other portions of the same province. I am confident that the joint work on the ground will enable me to present this broader view, as well as the local details, more successfully than I have been able to do in print. "In the meantime" I venture to hope that readers will "suspend their judgment on the questions raised" by the paper of Prof. Harrison and Mr. Jukes-Browne. J. W. SPENCER.

WASHINGTON, *December 19th*, 1902.

CLASSIFICATION OF THE LOWER CHALK OF NORTH GERMANY.

SIR,—There is a curious error in the Short Notice of Dr. A. von Koenen's paper "Ueber die Gliederung der norddeutschen Unteren Kreide" in your December number. The reviewer implies that the result of the learned author's revised classification is to regard the Aptien, Barrêmien, Hauterivien, and Valanginien as subdivisions of the Albien!

We know that the German Albien is sufficiently comprehensive; but it has not yet been stretched to this extent. It is merely that your reviewer, in his innocence, has been misled by finding the Albien standing a little apart in its place at the head of the column of 'stages'! G. W. LAMPLUGH.

14, HUME STREET, DUBLIN.

TELMATOSAURUS, NEW NAME FOR THE DINOSAUR *LIMNOSAURUS*.

SIR,—Professor G. B. Fletcher has had the great kindness to inform me that the name *Limnosaurus*, which I proposed in 1899 for a new

Dinosaurian, was preoccupied by Marsh for a crocodile (1871). I therefore propose to name the Dinosaur mentioned (Nopcsa, Denkschriften R. Akad. Wissensch. Wien, 1899) TELMATOSAURUS.

Baron F. NOPCSA, Jun.

VIENNA, *January 11th*, 1903.

GRANITE AND QUARTZ-VEINS.

SIR,—The paper by Mr. J. Lomas on "Quartz Dykes near Foxdale, Isle of Man," which appears in your January number (p. 34), raises an interesting question, and presents the argument in a cogent form. There can be no doubt that, on the fringe of a granite intrusion and in its apophyses, we sometimes find a gradual transition from normal granite, through various rocks which may be termed pegmatite, greisen, etc., to pure vein-quartz. Some phases of this transition are especially well displayed at Foxdale, a locality which I have already cited in this connection (Q.J.G.S., 1895, vol. li, pp. 143, 144), and which has now been described in detail by Mr. Lomas.

Closer inquiry is, however, necessary before we can be warranted in regarding such quartz-veins as igneous rocks in the ordinary sense. There are many indications, both from the geological and from the petrographical side, that the more siliceous products in question, and especially the pure quartz-veins, belong at most to the waning stage of igneous activity, when the temperature had fallen and the agency of water had become a more important factor. Dr. Sorby's well-known researches on fluid cavities, for instance, strongly support this view (Q.J.G.S., 1858, vol. xiv, pp. 471–475). But, further, there is sometimes reason to believe that, in these highly quartzose fringes and veins in very intimate connection with granite, a considerable part of the quartz has replaced felspar, and is therefore not strictly a primary mineral. One very clear example among others was described some years ago by Mr. Marr and myself on the edge of the Shap granite (Q.J.G.S., 1891, vol. xlvii, p. 285). Here distinct pseudomorphs of quartz after felspar put the question beyond doubt. In the greisens of Cornwall and Saxony, the beresite of the Urals, and such peculiar rocks as luxulyanite and trowlesworthite, the occurrence of special 'pneumatolytic' minerals like tin-stone, topaz, tourmaline, and fluor is equally convincing. We must recognize the possibility of a like origin for veins of quartz, or of quartz and mica, even where no direct evidence of replacement is preserved; and the existence of an igneous magma composed of pure, or nearly pure, silica cannot as yet be regarded as proved.

ALFRED HARKER.

ST. JOHN'S COLLEGE, CAMBRIDGE.
January 17th, 1903.

THE TERM 'HEMERA.'

SIR,—Mr. Jukes-Browne seems to be haunted by the good word 'stratigraphical.' In the January number he finds fault with my